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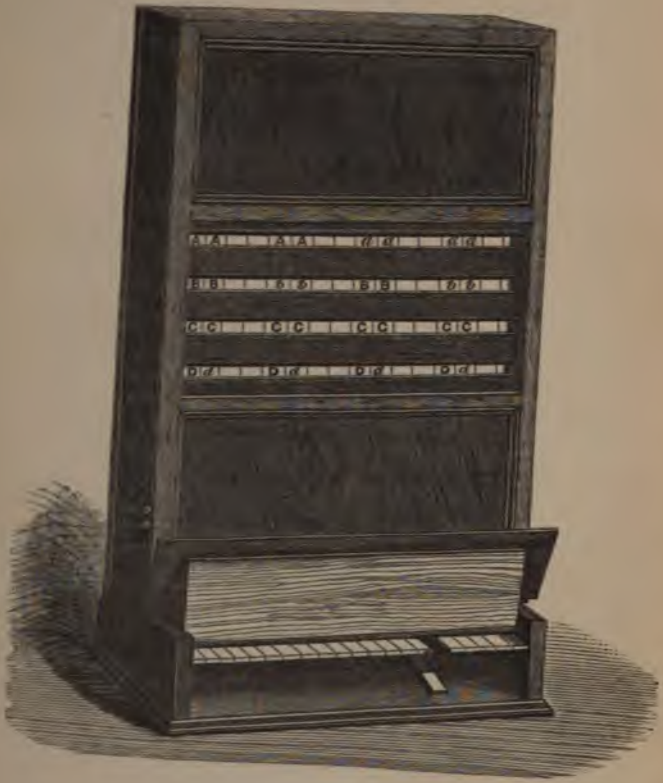

THE LOGICAL MACHINE.

THE PREDICATE-KEY C IS DEPRESSED.

THE PRINCIPLES OF SCIENCE:

A TREATISE ON LOGIC
AND
SCIENTIFIC METHOD.

BY

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PREFACE.

It may be truly asserted that the rapid progress of the physical sciences during the last three centuries has not been accompanied by a corresponding advance in the theory of reasoning. Physicists speak familiarly of Scientific Method, but they could not readily describe what they mean by that expression. Profoundly engaged in the study of particular classes of natural phenomena, they are usually too much engrossed in the immense and ever-accumulating details of their special sciences, to generalize upon the methods of reasoning which they unconsciously employ. Yet few will deny that these methods of reasoning ought to be studied, especially by those who endeavour to introduce scientific order into less successful and methodical branches of knowledge.

The application of Scientific Method cannot be restricted to the sphere of lifeless objects. We must sooner or later have strict sciences of those mental and social phenomena, which, if comparison be possible, are of more interest to us than purely material phenomena. But it is the proper course of reasoning to proceed from the known to the unknown—from the evident to the obscure—from the material and palpable to the subtle and refined. The physical sciences may therefore be properly made the practice-ground of the reasoning powers, because they furnish us with a great body of precise and successful investigations. In these sciences we meet with happy instances of unquestionable deductive reasoning, of extensive generalization, of happy prediction, of satisfactory verification, of nice calculation of probabilities. We can note how the slightest analogical clue has been followed up to a glorious discovery, how a rash generalization has at length been exposed, or a conclusive *experimentum crucis* has decided the long-continued strife between two rival theories.

In following out my design of detecting the general methods of inductive investigation, I have found that the more elaborate and interesting processes of quantitative induction have their necessary foundation in the simpler science of Formal Logic. The earlier, and probably by far the least attractive part of this work, consists, therefore, in a statement of the so-called Fundamental Laws of Thought, and of the all-important Principle of Substitution, of which, as I think, all reasoning is a development. The whole procedure of inductive inquiry, in its most complex cases, is foreshadowed in the combinational view of Logic, which arises directly from these fundamental principles. Incidentally I have described the mechanical arrangements by which the use of the important form called the Logical Abecedarium, and the whole working of the combinational system of Formal Logic, may be rendered evident to the eye, and easy to the mind and hand.

The study both of Formal Logic and of the Theory of Probabilities, has led me to adopt the opinion that there is no such thing as a distinct method of induction as contrasted with deduction, but that induction is simply an inverse employment of deduction. Within the last century a reaction has been setting in against the purely empirical procedure of Francis Bacon, and physicists have learnt to advocate the use of hypotheses. I take the extreme view of holding that Francis Bacon, although he correctly insisted upon constant reference to experience, had no correct notions as to the logical method by which, from particular facts, we educe laws of nature. I endeavour to show that hypothetical anticipation of nature is an essential part of inductive inquiry, and that it is the Newtonian method of deductive reasoning combined with elaborate experimental verification, which has led to all the great triumphs of scientific research.

In attempting to give an explanation of this view of Scientific Method, I have first to show that the sciences of number and quantity repose upon and spring from the simpler and more general science of Logic. The Theory of Probability, which enables us to estimate and calculate quantities of knowledge, is then described, and especial attention is drawn to the Inverse Method of Probabilities, which involves, as I conceive, the true principle of inductive procedure. No inductive conclusions are more than probable, and I adopt the opinion that the theory of probability is an essential part of logical method, so that the logical value of every inductive result must be determined consciously or unconsciously, according to the principles of the inverse method of probability.

The phenomena of nature are commonly manifested in quantities of time, space, force, energy, &c., and the observation, measurement, and analysis of the various quantitative conditions or results involved, even in a simple experiment, demand much employment of systematic procedure. I devote a book, therefore, to a simple and general description of the devices by which exact measurement is effected, errors eliminated, a probable mean result attained, and the probable error of that mean ascertained. I then proceed to the principal, and probably the most interesting, subject of the book, illustrating successively the conditions and precautions requisite for accurate observation, for successful experiment, and for the sure detection of the quantitative laws of nature. As it is impossible to comprehend aright the value of quantitative laws without constantly bearing in mind the degree of quantitative approximation to the truth probably attained, I have devoted a special chapter to the Theory of Approximation, and however imperfectly I may have treated this subject, I must look upon it as a very essential part of a work on Scientific Method.

It then remains to illustrate the sound use of hypothesis, to distinguish between the portions of knowledge which we owe to empirical observation, to accidental discovery, or to scientific prediction. Interesting questions arise concerning the accordance of quantitative theories and experiments, and I point out how the successive verification of an hypothesis by distinct methods of experiment yields conclusions approximating to but never attaining certainty. Additional illustrations of the general procedure of inductive investigations are given in a chapter on the Character of the Experimentalist, in which I endeavour to show, moreover, that the inverse use of deduction was really the logical method of such great masters of experimental inquiry as Newton, Huyghens, and Faraday.

In treating Generalization and Analogy, I consider the precautions requisite in inferring from one case to another, or from one part of the universe to another part, the validity of all such inferences resting ultimately upon the inverse method of probabilities. The treatment of Exceptional Phenomena appeared to afford an interesting subject for a further chapter illustrating the various modes in which an outstanding fact may eventually be explained. The formal part of the book closes with the subject of Classification, which is, however, very inadequately treated.

my mind, that serious misconceptions are entertained
y some scientific men as to the logical value of our know-
edge of nature. We have heard much of what has been
ptly called the Reign of Law, and the necessity and uni-
ormity of natural forces has been not uncommonly inter-
reted as involving the non-existence of an intelligent and
enevolent Power, capable of interfering with the course
f natural events. Fears have been expressed that the
ogress of Scientific Method must therefore result in dis-
ipating the fondest beliefs of the human heart. Even the
Utility of Religion' is seriously proposed as a subject of
iscussion. It seemed to be not out of place in a work on
cientific Method to allude to the ultimate results and
imits of that method. I fear that I have very imper-
ectly succeeded in expressing my strong conviction that
efore a rigorous logical scrutiny the Reign of Law will
rove to be an unverified hypothesis, the Uniformity of
Nature an ambiguous expression, the certainty of our
ientific inferences to a great extent a delusion. The
alue of science is of course very high, while the con-
usions are kept well within the limits of the data on
which they are founded, but it is pointed out that our

sophy which, building on a few material facts, presumes to assert that it has compassed the bounds of existence, while it nevertheless ignores the most unquestionable phenomena of the human mind and feelings.

I have to thank my colleague, Professor Barker, for carefully revising several of the sheets most abounding in mathematical considerations. It is approximately certain that in freely employing illustrations drawn from many different sciences, I have frequently fallen into errors of detail. In this respect I must throw myself upon the indulgence of the reader, who will bear in mind, as I hope, that the scientific facts are generally mentioned purely for the purpose of illustration, so that inaccuracies of detail will not in the majority of cases affect the truth of the general principles illustrated.

December 15th, 1873.

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THE PRINCIPLES OF SCIENCE.

CHAPTER I.

INTRODUCTION.

SCIENCE arises from the discovery of Identity amid Diversity. The process may be described in many different words, but our language must always imply the presence of one common and necessary element. In every act of inference or scientific method we are engaged about a certain identity, sameness, similarity, likeness, resemblance, analogy, equivalence or equality apparent between two objects. It is doubtful whether an entirely isolated phenomenon could present itself to our notice, since there must always be a contrast between object and object to awaken our consciousness. But in any case an isolated phenomenon could be studied to no useful purpose. The whole value of science consists in the power which it confers upon us of applying to one object the knowledge acquired from like objects; and it is only so far, therefore, as we can discover and register resemblances or differences that we can turn our observations to account.

Nature is a spectacle continually exhibited to our senses, in which phenomena are mingled in combinations of endless variety and novelty. Wonder fixes the mind's attention; memory stores up a record of each distinct impression, the powers of association bring forth the record when the like is felt again. By the higher faculties of judgment and reasoning the mind compares the new with the old, recognises essential identity, even when disguised by diverse circumstances, and expects to find again what was before experienced. It must be the ground of all reasoning and inference that *what is true of one thing will be true of its equivalent*, and that under carefully ascertained conditions *Nature repeats herself*.

Were this indeed a Chaotic Universe, the powers of mind employed in science would be useless to us. Did Chance wholly take the place of order, and did all phenomena come out, not of one same *Infinite Lottery*, to use Condorcet's expression, but out of lotteries ever changing in their conditions, there could be no reason to expect the like result in like circumstances. It is possible to conceive a world in which no two things should be associated more often, in the long run, than any other two things. The frequent conjunction of any two events would then be purely fortuitous, and if we expected conjunctions to recur continually we should be disappointed. In such a world we might recognise the same phenomenon as it appeared from time to time, just as we might recognise a marked ball as it was occasionally drawn from a ballot-box; but the approach of any one phenomenon would be in no way indicated by what had gone before, nor would it be at all a sign of what was to come after. In such a world knowledge would be no more than the memory of past coincidences, and the reasoning powers, if they existed at all, would give no clue to the nature of the present, and no presage of the future.

Happily the Universe in which we dwell is not the result of chance, and where chance seems to work it is our own deficient faculties which prevent us from recognising the operation of Law and of Design. In the material framework of this world, substances and forces present themselves in definite and stable combinations. All things are not in perpetual flux, as ancient philosophers held. Element remains element; iron changes not into gold, nor oxygen into hydrogen. With suitable precautions we can calculate upon finding the same thing again endowed with the same properties. The constituents of the globe, indeed, appear in almost endless combinations; but each combination bears its fixed character, and when resolved is found to be the compound of definite substances. Misapprehensions must continually occur, owing to the limited extent of our experience. We can never have examined and registered possible existences so thoroughly as to be sure that no new ones will occur and frustrate our calculations. The same outward appearances may cover any amount of hidden differences which we have not yet suspected. To the variety of substances and powers diffused through nature at its creation, we must not suppose that our brief experience can assign a limit; and the necessary imperfection of our knowledge should be ever borne in mind.

Yet there is much to give us confidence in science. The wider our experience, the more minute our examination of the globe, the greater the accumulation of well-reasoned knowledge,—the fewer must become the failures of inference compared with the successes. Exceptions to the prevalence of Law are gradually reduced to Law themselves. Certain deep similarities have been detected among the objects around us, and have never yet been found wanting. As the means of examining distant parts of the universe have been acquired, those similarities have been traced there as here. Other worlds and stellar systems may be almost incomprehensively different from ours in magnitude, *condition and disposition* of parts, and yet we detect there the same elements of which our own limbs are composed. The same natural laws can be detected in operation in every part of the universe within the scope of our instruments; and doubtless these laws are obeyed irrespective of distance, time and circumstance.

It is the prerogative of Intellect to discover what is uniform and unchanging in the phenomena around us. So far as object is different from object, knowledge is useless and inference impossible. But so far as object resembles object, we can pass from one to the other. In proportion as resemblance is deeper and more general, the commanding powers of knowledge become more wonderful. Identity in one or other of its phases is thus always the bridge by which we pass in inference from case to case; and it is my purpose in this treatise to trace out the various forms in which the one same process of reasoning presents itself in the ever-growing achievements of Scientific Method.

The Powers of Mind concerned in the Creation of Science.

It is no part of the purpose of this work to investigate the nature of mind, except so far as its powers are requisite to the formation of Science. In this place I need only point out that the mental powers engaged in knowledge are probably three in number. They are substantially as Mr. Bain has stated them[a]:—

1. The Power of Discrimination.
2. The Power of Detecting Identity.
3. The Power of Retention.

We exert the first power in every act of perception. Hardly can we have a sensation or feeling unless we discriminate it from something else which preceded.

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[a] 'The Senses and the Intellect,' Second Ed., pp. 5, 325, &c.

Consciousness would almost seem to consist in the break between one state of mind and the next, just as an induced current of electricity arises from the beginning or the ending of the primary current. We are always engaged in discrimination; and the rudiment of thought which exists in the lower animals probably consists in their power of feeling difference and being agitated by its occurrence.

But had we power of discrimination only, Science could not be created. To know that one feeling differs from another gives purely negative information. It cannot teach us what will happen. Each sensation would stand out distinct from any other, and there would be no tie, no bridge of affinity between them. We want a unifying power by which the present and the future may be linked to the past; and this seems to be accomplished by a different power of mind. Francis Bacon has pointed out that different men possess in very different degrees the powers of discrimination and identification. It may be said indeed that discrimination necessarily implies the opposite process of identification; and so it doubtless does in superficial points. But there is a rare property of mind which consists in penetrating the disguise of variety and seizing the common elements of sameness; and it is this property which furnishes the true measure of intellect. The very name of intellect (*interligo*) expresses the action, not of separating, but of uniting and binding together the particular and various into the general and like. Logic is but another name for the same process[b], the peculiar work of reason; and Plato said of this unifying power, that if he met the man who could detect *the one in the many*, he would follow him as a god.

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[b] Max Müller, 'Lectures on Language,' Second Series, vol. ii. p. 63.

Laws of Identity and Difference.

At the basis of all thought and science must lie the laws which express the very nature and conditions of the discriminating and identifying powers of mind. These are the so-called Fundamental Laws of Thought, usually stated as follows:—

1. The Law of Identity. *Whatever is, is.*
2. The Law of Contradiction. *A thing cannot both be and not be.*
3. The Law of Duality. *A thing must either be or not be.*

The first of these statements may perhaps be regarded as a description of identity itself, if so fundamental a notion can admit of description. A thing at any moment is perfectly identical with itself, and if any person were unaware of the meaning of the word 'identity' we could not better describe it than by such an example.

The second law points out that contradictory attributes can never be joined together. The same object may vary in its different parts; here it may be black, and there white; at one time it may be hard and at another time soft: but at the same time and place an attribute cannot be both present and absent. Aristotle truly described this law as the first of all axioms[c]—one of which we need not seek for any demonstration. All truths cannot be proved, otherwise there would be an endless chain of demonstration; and it is in self-evident truths like this that we find the fittest foundation.

The third of these laws completes the other two. It asserts that at every step there are two possible alternatives—presence or absence, affirmation or negation. Hence I propose to name this law the Law of Duality, for it gives to all the formulæ of reasoning a dual character. It asserts also that between presence or absence, existence or non-existence, affirmation or negation, there is no third alternative. As Aristotle said, there can be no mean between opposite assertions: we must either affirm or deny. Hence the somewhat inconvenient name by which it has been generally known—The Law of Excluded Middle.

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[c] 'Metaphysics,' Bk. III. chap. iii. 9–12.

It may be held that these laws are not three independent and distinct laws, they rather express three different aspects of the same truth, and each law doubtless presupposes and implies the other two. But it has not hitherto been found possible to state these characters of identity and difference in less than the three-fold formula. The reader may perhaps desire some information as to the mode in which these laws have been stated, or the way in which they have been regarded, by philosophers in different ages of the world. Abundant information on this and many other points of logical history will be found in Ueberweg's 'System of Logic,' of which an excellent translation has been published by Mr. T. M. Lindsay[d]. I must confess however that the history of logical doctrines has seemed to me one of the most confusing and least beneficial studies in which a person can engage; and over-abundant attention perhaps has been paid to it by Hamilton, Mansel, and many German logicians.

The Nature and Authority of the Laws of Identity and Difference.

I must at least allude to the profoundly difficult question concerning the nature and authority of these

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[d] Ueberweg's 'System of Logic,' transl. by Lindsay, London, 1871, pp. 228–281.

Laws of Identity or Difference. Are they Laws of Thought or Laws of Things? Do they belong to mind or to material nature? On the one hand it may be said that science is a purely mental existence, and must therefore conform to the laws of that which formed it. Science is in the mind and not in the things, and the properties of mind are therefore all important. It is true that these laws are verified in the observation of the exterior world; and it would seem that they might have been gathered and proved by generalisation, had they not already been in our possession. But on the other hand, it may well be urged that we cannot prove these laws by any process of reasoning or observation, because the laws themselves are presupposed, as Leibnitz acutely remarked, in the very notion of a proof. They are the prior conditions of all thought and all knowledge, and even to question their truth is to allow them true. Hartley ingeniously refined upon this argument, remarking that if the fundamental laws of logic be not certain, there must exist a logic of a second order whereby we may determine the degree of uncertainty: if the second logic be not certain, there must be a third, and so on *ad infinitum*. Thus we must suppose either that absolutely certain laws of thought exist, or that there is no such thing as certainty whatever [e].

Logicians, indeed, appear to me to have paid insufficient attention to the fact that mistakes in reasoning are always likely to occur. The Laws of Thought are often called necessary laws, that is, laws which cannot but be obeyed. Yet as a matter of fact who is there that does not often fail to obey them? They are the laws which the mind ought to obey rather than what it always does obey. Our thoughts cannot be the criterion of truth, for we often have to acknowledge mistakes in arguments of very moderate complexity, and we sometimes only discover our mistakes by a collision between our mental expectations and the events of objective nature.

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[e] Hartley on Man, vol. i. p. 359.

Mr. Herbert Spencer holds that the laws of logic are objective laws[f], and he regards the mind as being in a state of constant education, each act of false reasoning or miscalculation leading to results which are likely to prevent similar mistakes from being again committed. I am quite inclined to accept such ingenious views; but at the same time it is necessary to distinguish between the accumulation of knowledge and experience, and the constitution of the mind which allows of the acquisition of knowledge. Before the mind can perceive or reason at all it must have the conditions of thought impressed upon it. Before a mistake can be committed, the mind must clearly distinguish the mistaken conclusion from all other assertions. Are not the Laws of Identity and Difference the prior conditions of all consciousness and all existence? Must they not hold true, alike of things material and immaterial? and if so, can we say that they are only subjectively true or objectively true? I am inclined, in short, to regard them as true both 'in the nature of thought and things,' as I expressed it in my first logical essay[g], and I hold that they belong to the common basis of all existence. But this is one of the most profound and difficult questions of psychology and metaphysics which can be raised, and it is hardly one for the logician to decide. As the mathematician does not inquire into the nature of unity and plurality, but developes the formal laws of plurality, so the logician, as I conceive, must assume the truth of the Laws of

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[f] 'Principles of Psychology,' Second Ed., vol. ii. p. 86.

[g] 'Pure Logic, or the Logic of Quality apart from Quantity,' London (Stanford), 1864, pp. 10, 16, 22, 29, 36, &c.

Identity and Difference, and occupy himself in developing the variety of forms of reasoning in which their truth may be manifested.

Again, I need hardly dwell upon the question whether logic treats of language, notions, or things. As reasonably might we debate whether a mathematician treats of symbols, quantities, or things. A mathematician certainly does treat of symbols, but only as the instruments whereby to facilitate his reasoning concerning quantities; and as the axioms and rules of mathematical science must be verified in concrete objects in order that the calculations founded upon them may have any validity or utility, it follows that the ultimate objects of mathematical science are the things themselves. In like manner I conceive that the logician treats of language so far as it is essential for the embodiment and exhibition of thought. Even if reasoning can take place in the inner consciousness of man without the use of any signs, at any rate it cannot become the subject of discussion until by some system of material signs it is manifested to other persons. The logician then uses words and symbols as instruments of reasoning, and leaves the nature and peculiarities of existing language to the grammarian. But signs again must correspond to the thoughts and things expressed, in order that they shall serve their intended purpose. We may therefore say that logic treats ultimately of thoughts and things, and immediately of the signs which stand for them. Signs, thoughts and exterior objects may be regarded as parallel and analogous series of phenomena, and to treat one series is equivalent to treating either of the other series[h].

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[h] See also 'Elementary Lessons in Logic,' Second Ed., p. 10.

The Process of Inference.

The fundamental action of our reasoning faculties consists in inferring or carrying to a new instance of a phenomenon whatever we have previously known of its like, analogue, equivalent or equal. Sameness or identity presents itself in all degrees, and is known under various names; but the great rule of inference embraces all degrees, and affirms that *so far as there exists sameness, identity or likeness, what is true of one thing will be true of the other.* The great difficulty of reasoning doubtless consists in ascertaining that there does exist a sufficient degree of likeness or sameness to warrant an intended inference; and it will be our main task to investigate the conditions under which the inference is valid. In this place I wish to point out that there is something common to all acts of inference however different their apparent forms. The one same rule lends itself to the most diverse applications.

The simplest possible case of inference, perhaps, occurs in the use of a *pattern*, *example*, or, as it is commonly called, a *sample.* To prove the exact similarity of two portions of commodity, we need not bring one portion beside the other. It is sufficient that we cut a sample which exactly represents the texture, appearance, and general nature of one portion, and according as this sample agrees or not with the other, so will the two portions of commodity agree or differ. Whatever is true as regards the colour, texture, density, material of the sample will be true of the goods themselves. In such cases likeness of quality is the condition of inference.

Exactly the same mode of reasoning holds true of magnitude and figure. To compare the size of two objects, we need not lay *them alongside* each other. A staff, string, or other kind of measure may be employed to represent the length of one object, and according as it agrees or not with the other, so must the two objects agree or differ. In this case the proxy or sample represents length; but the fact that lengths can be added and multiplied renders it unnecessary that the proxy should always be as large as the object. Any standard of convenient length, such as a common foot-rule, may be made the medium of comparison. The height of a church in one town may be carried to that in another, and objects existing immoveably at opposite sides of the earth may be vicariously measured against each other. We obviously employ the rule that whatever is true of a thing as regards its length, is true of its equal.

To every other simple phenomenon in nature the same principle of substitution is applicable. We may compare weights or densities or degrees of hardness, and all other qualities, in like manner. To ascertain whether two sounds are in unison we need not compare them directly, but a third sound may be the go-between. If a tuning-fork is in unison with the middle C of York Minster organ, and we afterwards find it to be in unison with the same note of the organ in Westminster Abbey, then it follows that the two organs are tuned in unison. The rule of inference now is that what is true, as regards pitch, of the tuning-fork, is true of any sound in unison with it.

The skilful employment of this substitutive process enables us to make measurements beyond the powers of our senses. No one can count the vibrations, for instance, of an organ pipe. But we can construct an instrument called the *syren*, so that while producing a sound of any pitch it shall register the number of vibrations constituting the sound. Adjusting the sound of the syren in unison with an organ pipe, we measure indirectly the number of vibrations belonging to a sound of that pitch. To measure a sound of the same pitch is as good as to measure the sound itself.

Sir David Brewster, in a somewhat similar manner, succeeded in measuring the refractive index of irregular fragments of transparent minerals. It was a troublesome, and sometimes impracticable work to grind the minerals into prisms, so that their powers of refracting light could be directly observed ; but he fell upon the ingenious device of forming a liquid possessing exactly the same refractive power as the transparent fragment under examination. The moment when this equality was attained could be known by the fragments ceasing to reflect or refract light when immersed in the liquid, so that they became almost invisible in it. The refractive power of the liquid being then measured gives that of the solid ; and a more beautiful instance of representative measurement, depending immediately upon the principle of inference, could not be found[i].

Throughout the various logical processes which we are about to consider—Deduction, Induction, Generalisation, Analogy, Classification, Quantitative Reasoning—we shall find the one same principle operating in a more or less disguised form.

Deduction and Induction.

The processes of inference always depend on the one same method of substitution ; but they may nevertheless be distinguished according as the results are inductive or deductive. As generally stated, deduction consists in passing from more general to less general truths; induction is the contrary process from less to more general truths. We may however describe the difference in another manner. In deduction we are engaged in developing the consequences of a law or identity. We learn the meaning, contents, results or inferences, which attach to any given proposition. Induction is the exactly inverse process. Given certain results or consequences, we are required to discover the general law from which they flow.

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[i] Brewster, 'Treatise on New Philosophical Instruments,' p. 273. See also Whewell, 'Philosophy of the Inductive Sciences,' vol. ii. p. 355 ; Tomlinson, 'Philosophical Magazine,' Fourth Series, vol. xl. p. 328 ; Tyndall, in Youman's '*Modern Culture,' p. 16.*

In a certain sense all knowledge is inductive. We can only learn the laws and relations of things in nature by observing those things. But the knowledge gained from the senses is knowledge only of particular facts, and we require some process of reasoning by which we may construct out of the facts the laws obeyed by them. Experience gives us the materials of knowledge: induction digests those materials, and yields us general knowledge. Only when we possess such knowledge, in the form of general propositions and natural laws, can we usefully apply the reverse process of deduction to ascertain the exact information required at any moment. In its ultimate origin or foundation, then, all knowledge is inductive—in the sense that it is derived by a certain inductive reasoning from the facts of experience.

But it is nevertheless true,—and this is a point to which insufficient attention has been paid,—that all reasoning is founded on the principles of deduction. I call in question the existence of any method of reasoning which can be carried on without a knowledge of deductive processes. I shall endeavour to show that *induction is really the inverse process of deduction.* There is no mode of ascertaining the laws which are obeyed in certain phenomena, except we previously have the power of determining what results would follow from a given law. Just as the *process* of division necessitates a prior knowledge of multiplication, or the integral calculus rests upon the observation and remembrance of the results of the differential calculus, so induction requires a prior knowledge of deduction. An inverse process is the undoing of the direct process. A person who enters a maze must either trust to chance to lead him out again, or he must carefully notice the road by which he entered. The facts furnished to us by experience are a maze of particular results; we might by chance observe in them the fulfilment of a law, but this is scarcely possible, unless we thoroughly learn the effects which would attach to any particular law.

Accordingly, the importance of deductive reasoning is doubly supreme. Even when we gain the results of induction they would be of little or no use without we could deductively apply them. But before we can gain them at all we must understand deduction, since it is the inversion of deduction which constitutes induction. Our first task then, in this work, must be to trace out fully the nature of identity in all its forms of occurrence. Having given any series of propositions we must be prepared to develop the whole meaning embodied in them, and the whole of the consequences which flow from them.

Symbolic Expression of Logical Inference.

In developing the results of the Principle of Inference we require to use an appropriate language of signs. It would indeed be quite possible to explain the processes of reasoning merely by the use of words found in the ordinary grammar and dictionary. Special examples of reasoning, too, may seem to be more readily apprehended than general and symbolic forms. But it has been abundantly proved in the mathematical sciences that the attainment of truth depends greatly upon the invention of a clear, brief, and appropriate *system of symbols.* *Not only* is such a language convenient, but it is essential to the expression of those general truths which are the very soul of science. To apprehend the truth of special cases of inference does not constitute logic; we must apprehend them as cases of more general truths. The object of all science is the separation of what is common and general from what is accidental and different. In a system of logic, if anywhere, we should esteem this generality, and strive to exhibit clearly what is similar in very diverse cases. Hence the great value of *general symbols* by which we can represent the form and character of a reasoning process, disentangled from any consideration of the special subject to which it is applied.

The signs required in logic are of a very simple kind. As every sameness or difference must exist between two things or notions, we need signs or terms to indicate the things or notions compared, and other signs to denote the relation between them. We shall need, then, (1) symbols for terms, (2) a symbol for sameness, (3) a symbol for difference, and (4) one or two symbols to take the place of conjunctions.

Ordinary nouns substantive, such as *Iron, Metal, Electricity, Undulation,* might serve as terms, but for the reasons explained above it is better to adopt blank letters, devoid of special signification, such as A, B, C, D, E, &c. Each letter must be understood to represent a noun, and, so far as the conditions of the argument allow, *any noun.* Just as in Algebra, x, y, z, p, q, r, &c. are used for *any quantities,* undetermined or unknown, except when the special conditions of the problem are taken into account, so will our letters stand for undetermined or unknown things.

These letter-terms will be used indifferently for nouns substantive and adjective. Between these two kinds of nouns there may be important differences in a metaphysical or grammatical point of view. But grammatical usage readily sanctions the free conversion of adjectives into substantives, and *vice versâ;* we may avail ourselves of this latitude without in any way prejudging the metaphysical difficulties which may be involved. Here, as throughout this work, I shall devote my attention to truths which I can exhibit in a clear and formal manner, believing that, in the present condition of logical science, this will lead to much greater advantage than discussion upon the metaphysical questions which may underlie any part of the subject.

Every noun or term denotes an object, and usually implies the possession by that object of certain qualities or circumstances common to all the objects denoted. There are certain terms, however, which imply the absence of qualities or circumstances attaching to other objects. It will be convenient to employ a special mode of indicating these *negative terms*, as they are called. If the general name *A* denotes an object or class of objects possessing certain defined qualities, then the term *Not-A* will denote any object which does not possess the whole of those qualities: in short, *Not-A* is the sign for anything which differs from *A* in regard to any one or more of the assigned qualities. If *A* denote 'transparent object,' *Not-A* will denote 'not transparent object.' Brevity and facility of writing and reading are of no slight importance in a system of notation, and it will therefore be desirable to substitute for the negative term *Not-A* a briefer mode of expression. The late Prof. de Morgan represented negative terms by small Roman letters, or sometimes by small italic letters[k], and as the latter seem to be highly convenient, I shall use *a, b, c, d, e, . . . p, q, r,* &c., as the negative terms corresponding to A, B, C, D, E, . . . P, Q, R, &c. Thus if A means 'fluid,' *a* will mean 'not-fluid,' and so on.

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[k] '*Formal Logic,*' *p. 38.*

Expression of Identity and Difference.

To denote the relation of sameness or identity I unhesitatingly adopt the sign =, so long used by mathematicians to denote equality. This symbol was originally appropriated by Robert Recorde in his 'Whetstone of Wit,' to avoid the tedious repetition of the words 'is equal to'; and he chose a pair of parallel lines, because no two things can be more equal[l]. The meaning of the sign has however been gradually extended beyond that of common equality; mathematicians have themselves used it to indicate equivalency of operations. The force of analogy has been so great that writers in all other branches of science have more or less employed the same sign. The philologist indicates by it equivalency of meaning of words: chemists adopt it to signify the identity in kind and equality in weight of the elements which form two different compounds. Not a few logicians, for instance Ploucquet, Condillac[m], George Bentham[n], Boole, have employed it as the copula of propositions. Prof. de Morgan declined to use it for this purpose, but still further extended its meaning so as to include the equivalency of a proposition with the premises from which it can be inferred[o], and Herbert Spencer has applied it in a like manner[p].

Many persons may think that the choice of a symbol is a matter of slight importance or of mere convenience, but I hold that the common use of this sign = in so many different meanings is really founded upon a generalisation of the widest character and of the greatest importance—one indeed which it is a principal object of this work to endeavour to explain. The employment of the same sign in different cases would be wholly unphilosophical unless there were some real analogy between its diverse meanings. If such analogy exist, it is not only allowable, but highly desirable and even imperative, to use the symbol of equivalency with a generality of meaning corresponding to the generality of the principles involved. Accordingly Prof. de Morgan's refusal to use the symbol in logical propositions indicated his opinion that there was a want of analogy between logical propositions and mathematical equations. I use the sign because I hold the contrary opinion.

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[l] Hallam's 'Literature of Europe,' First Ed. vol. ii. p. 444.

[m] Condillac, 'Langue des Calculs,' p. 157.

[n] 'Outline of a New System of Logic,' London, 1827, pp. 133, &c.

[o] 'Formal Logic,' pp. 82, 106. In his later work, 'The Syllabus of a New System of Logic,' he discontinued the use of the sign.

[p] '*Principles* of Psychology,' Second Ed., vol. ii. pp. 54, 55.

I conceive that the sign = always denotes some form or degree of sameness or equivalency, and the particular form is usually indicated by the nature of the terms joined by it. Thus '6720 pounds = 3 tons' is evidently an equation of quantities. The formula — × — = + expresses the equivalency of operations. 'Exogens = Dicotyledons' is a logical identity expressing a profound truth concerning the character of vegetables.

We have great need in logic of a distinct sign for the copula, because the little verb *is*, hitherto used both in logic and ordinary discourse, is thoroughly ambiguous. It sometimes denotes identity, as in 'St. Paul's is the chef-d'œuvre of Sir Christopher Wren,' but it more commonly indicates inclusion of class within class, or partial identity, as in 'Bishops are members of the House of Lords.' This latter relation involves identity, but requires careful discrimination from simple identity, as will be shown further on.

When with this sign of equality we join two nouns or logical terms, as in

Hydrogen = The least dense element,

we signify that the *object or group of objects* denoted by one term is identical with that denoted by the other i everything except the names. The general formula

$$A = B$$

must be taken to mean that A and B are symbols for th same object or group of objects. This identity may sometimes arise from the mere imposition of names, but i may also arise from the deepest laws of the constitutio of nature; as when we say

Gravitating matter = Matter possessing inertia,
Exogenous plants = Dicotyledonous plants,
Plagihedral quartz crystals = Quartz crystals rotating the plane of polarisation of light.

We shall need carefully to distinguish between relation of terms which can be modified at our own will and thos which are fixed as expressing the laws of nature; but a present we are considering only the mode of expression.

We may sometimes, but much less frequently, require symbol to indicate difference or the absence of complet sameness. For this purpose we may generalise in lik manner the symbol $\sim$, which was introduced by Wallis t signify difference between two numbers or quantities The general formula

$$B \sim C$$

denotes that B and C are the names of some two object or groups of objects which are not identical with eac other. Thus we may say

Acrogens $\sim$ Flowering plants.
Snowdon $\sim$ The highest mountain in Great Britain.

I shall also occasionally use the sign $\backsim$ to signify in th most general manner the existence of any relation betwee the two terms connected by it. Thus $\backsim$ might mean no only the relations of equality or inequality, sameness o difference, but any special relation of time, place, size causation, &c. in which one thing may stand to anothe By $A \backsim B$ I mean, then, any two objects of thought re*lated to each other* in any matter whatsoever.

General Formula of Logical Inference.

The one supreme rule of inference consists, as I have said, in the direction to affirm of anything whatever is known of its like, equal or equivalent. The *Substitution of Similars* is a phrase which seems aptly to express the power of mutual replacement existing between any two objects which are to a sufficient degree like or equivalent. It is a matter for further investigation to point out when and for what purposes a degree of similarity less than complete identity is sufficient to warrant substitution. For the present we think only of the exact sameness expressed in the form

$$A = B.$$

Now if we take the letter C to denote any third conceivable object, and use the sign $\backsim$ in its stated meaning of *indefinite relation*, then the general formula of all inference may be thus exhibited:—

From $A = B \backsim C$

we may infer $A \backsim C$

or, in words—*In whatever relation a thing stands to a second thing, in the same relation it stands to the like or equivalent of that second thing.* The identity between A and B allows us indifferently to place A where B was or B where A was, and there is no limit to the variety of special meanings which we can bestow upon the signs used in this formula consistently with its truth. Thus if we first specify only the meaning of the sign $\backsim$, we may say that if *C is the weight of B*, then *C is also the weight of A*. Similarly

If C is the father of B, C is the father of A;
If C is a fragment of B, C is a fragment of A;
If C is a quality of B, C is a quality of A;
If C is a species of B, C is a species of A;
If C is the *equal of B, C is the equal of A*;

and so on ad infinitum.

We may also endow with special meanings the letter-terms A, B and C, and the process of inference will never be false. Thus let the sign ∽ mean 'is height of,' and let

A = Snowdon,
B = Highest mountain in England or Wales,
C = 3590 feet;

then it obviously follows that since '3590 feet is the height of Snowdon,' and 'Snowdon = the highest mountain in England or Wales,' then '3590 feet is the height of the highest mountain in England or Wales.'

One result of this general process of inference is that we may in any aggregate or complex whole replace any part by its equivalent without altering the whole. To alter is to make a difference, but if in replacing a part I make no difference, there is no alteration of the whole. Many inferences which have been very imperfectly included in logical formulæ at once follow. I remember the late Prof. de Morgan remarking that all Aristotle's logic could not prove that 'Because a horse is an animal, the head of a horse is the head of an animal.' I conceive that this amounts merely to replacing in the complete notion *head of a horse*, the term 'horse' by its equivalent *some animal* or *an animal*. Similarly, since

The Lord Chancellor = The Speaker of the House of Lords,

it follows that

The death of the Lord Chancellor = The death of the Speaker of the House of Lords;

and any event, circumstance or thing which stands in a certain relation to the one will stand in like relation to the other. Milton reasons in this way when he says, in his Areopagitica, 'Who kills a man, kills a reasonable creature, God's image.' If we may suppose him to mean

God's image = man = some reasonable creature, it follows that 'The killer of a man is the killer of some reasonable creature,' and also 'The killer of God's image.'

This replacement of equivalents may be repeated over and over again to any extent. Thus if *person* is identical in meaning with *individual*, it follows that

Meeting of persons = meeting of individuals ;

and if *assemblage* = *meeting*, we may make a new replacement and show that

Meeting of persons = assemblage of individuals.

We may in fact found upon this principle of substitution a most general axiom in the following terms[q] :—

Same parts samely related make same wholes.

If, for instance, exactly similar bricks be used to build two houses, and they be similarly placed in each house, the two houses must be similar. There are millions of cells in a human body, but if each cell of one person were represented by an exactly similar cell similarly placed in another body, the two persons would be undistinguishable, and would be only *numerically* different. It is upon this principle, as we shall see, that all accurate processes of measurement depend. If for a weight in a scale of a balance we substitute another weight, and the equilibrium remains entirely unchanged, then the weights must be exactly equal. The general test of equality is substitution. Objects are equally bright when on replacing one by the other the eye perceives no difference. Two objects are equal in dimensions when tested by the same gauge they fit in the same manner. Generally speaking, two objects are alike so far as when substituted one for another no alteration is produced, and *vice versâ* when alike no alteration is produced by the substitution.

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[q] 'Pure Logic, or the Logic of Quality,' p. 14.

The Propagating Power of Identity.

The relation of identity or sameness in all its degrees is reciprocal. So far as things are alike, either may be substituted for the other; and this may perhaps be considered the very meaning of the relation. But it is well worth notice that there is in identity a peculiar power of extending itself among all the things which are identical. To render a number of things similar to each other we need only render them similar to one standard object. Each coin struck from a pair of dies not only exactly resembles the matrix or original pattern from which the dies were struck, but exactly resembles every other coin manufactured from the same original pattern. Among a million such coins there are not less than 499,999,500,000 of *pairs of coins* exactly resembling each other. Similars to the same are similars to all. It is one great advantage of printing that all copies of a document taken from the same type are necessarily identical each with each, and whatever is true of one copy will be true of every copy. Similarly, if fifty rows of pipes in an organ be tuned in perfect unison with one row, usually the Principal, they must be in unison with each other. Identity can also reproduce or propagate itself *ad infinitum;* for if a number of tuning-forks be adjusted in perfect unison with one standard fork, all instruments tuned to any one fork will agree with any instrument tuned to any other fork. Standard measures of length, capacity, or weight, or any other measureable quality, are propagated in the same manner. So far as copies of the original standard, or copies of copies, or copies again of those copies, are accurately executed, they must all agree each with every other.

It is the power of mutual substitution which gives such great value to the modern methods of mechanical construction, according to which all the parts of a machine are exact facsimiles of a fixed pattern. All the rifles used in the British army are constructed on the interchangeable system, so that any one part of any one rifle can be substituted indifferently for the same part of another. A bullet fitting one rifle will fit all others of the same bore. Sir J. Whitworth has extended the same system to the screws and screw-bolts used in connecting together the parts of machines, by establishing a series of standard screws.

Anticipations of the Principle of Substitution.

In such a subject as logic it is hardly possible to put forth any opinions or principles which have not been in some degree previously entertained. The germ at least of every doctrine will be found in earlier writers, and novelty must arise chiefly in the mode of harmonising and developing ideas. When I first proposed to employ the process and name of *substitution* in logic[r], I believe that I was led to do so from analogy with the familiar mathematical process of substituting for a symbol its value as given in an equation. In writing my first logical essay I had a most imperfect conception of the importance and generality of the process, and I described, as if they were of equal importance, a number of other laws which now seem to be but particular cases of the one general rule of substitution.

My second essay, the Substitution of Similars, was written shortly after I had become aware of the great simplification which may be effected by a proper application of the principle of substitution. I was not then acquainted with the fact that the German logician

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[r] 'Pure Logic,' pp. 18–19.

Beneke had employed the principle of substitution, and had used the word itself in forming a theory of the syllogism. My imperfect acquaintance with the German language had prevented me from acquiring a complete knowledge of Beneke's views, but there is no doubt that Mr. Lindsay is right in saying that he, and probably other previous logicians, were in some degree familiar with the principle[s]. Even Aristotle's dictum may be regarded as an imperfect statement of the principle of substitution; and, as I have pointed out, we have only to modify that dictum in accordance with the quantification of the predicate in order to arrive at the complete process of substitution[t]. The Port-Royal logicians appear to have entertained nearly equivalent views, for they considered that all moods of the syllogism might be reduced under one general principle[u]. Of two premises they regard one as the *containing proposition* (propositio continens), and the other as the *applicative proposition.* The latter proposition must always be affirmative, and represents that by which a substitution is made; the former may or may not be negative, and is that in which a substitution is effected. They also show that this method will embrace certain cases of complex reasoning which had no place in the Aristotelian syllogism. Their views probably constitute the greatest improvement in logical doctrine made up to that time since the days of Aristotle. But a true reform in logic must consist, not in explaining the syllogism in one way or another, but in doing away with all the narrow restrictions of the Aristotelian system, and in showing that there exists an indefinite extent of logical arguments immediately deducible from the principle of substitution of which the ancient syllogism forms but a small and not even the most important part.

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[s] Ueberweg's 'System of Logic,' transl. by Lindsay, pp. 442–446, 571, 572.

[t] 'Substitution of Similars,' p. 9.

[u] 'Port-Royal Logic,' transl. by Spencer Baynes, pp. 212–219. Part III. chap. x. and xi.

The Logic of Relatives.

There is a difficult and important branch of logic which may be called the Logic of Relatives. If I argue, for instance, that because Daniel Bernoulli was the son of John, and John the brother of James, therefore Daniel was the nephew of James, it is not possible to prove this conclusion by any simple logical process. We require at any rate to assume that the son of a brother is a nephew. A simple logical relation is that which exists between properties and circumstances of the same object or class. But objects and classes of objects may also be related according to all the properties of time and space. I believe it may be shown, indeed, that where an inference concerning such relations is drawn, a process of substitution is really employed and an identity must exist; but I will not undertake to prove the assertion in this work. The relations of time and space are logical relations of a complicated character demanding much abstract and difficult investigation. The subject has been treated with such great ability by Professors Peirce[x], De Morgan[y], Ellis[z], and Harley, that I will not in the present work attempt any review of their writings, but merely refer to the publications in which they are to be found.

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[x] 'Description of a Notation for the Logic of Relatives, resulting from an Amplification of the Conceptions of Boole's Calculus of Logic.' By C. S. Peirce. 'Memoirs of the American Academy,' vol. ix. Cambridge, U.S., 1870.

[y] 'On the Syllogism, No. IV, and on the Logic of Relations.' By Augustus De Morgan. 'Transactions of the Cambridge Philosophical Society,' vol. x. part ii. 1860.

[z] 'Observations on Boole's Laws of Thought.' By the late R. Leslie Ellis; communicated by the Rev. Robert Harley, F.R.S. 'Report

of the British Association,' 1870. 'Report of Sections,' p. 12. Also, 'On Boole's Laws of Thought.' By the Rev. Robert Harley, F.R.S., ibid. p. 14.

CHAPTER II.

TERMS.

EVERY proposition expresses the resemblance or difference of the things denoted by its terms. As reasoning or inference treats of the relation between two or more propositions, so a proposition consists in a relation between two or more terms. In the portion of this work which treats of deduction it will be convenient to follow the usual order of exposition, and consider in succession the various kinds of terms, propositions, and arguments, and we commence in this chapter with terms.

The simplest and most palpable meaning which can belong to a term consists of some single material object, such as Westminster Abbey, the Sun, Sirius, Stonehenge, &c. It is probable that in the earliest stages of intellect only concrete and palpable things are the objects of thought. The youngest child knows the difference between a hot and a cold body. The dog can recognise his master among a hundred other persons, and animals of much lower intelligence know and discriminate their haunts. In all such acts there is judgment concerning the likeness or unlikeness of physical objects, but there is little or no power of analysing each object and regarding it as a group of qualities or circumstances.

The dignity of intellect begins with the power of separating points of agreement from those of difference. Comparison of two *objects may lead us to* perceive that they are at once like and unlike. Two fragments of rock may differ entirely in outward form, yet they may have the same colour, hardness, and texture. Flowers which agree in colour may differ in odour. The mind learns to regard each object as an aggregate of qualities, and acquires the power of dwelling at will upon one or other of those qualities to the exclusion of the rest. Logical abstraction, in short, comes into play, and the mind becomes capable of reasoning, not merely about objects which are physically complete and concrete, but about things which may be thought of separately in the mind though they exist not separately in nature. We can think of the hardness of a rock, or the colour of a flower, and thus produce abstract notions, denoted by abstract terms which will form a subject for further consideration.

At the same time arise general notions and classes of objects. We cannot fail to observe that the quality *hardness* exists in many objects, for instance in many fragments of rock; and mentally joining these we create the class *hard object*, which will include, not only the actual objects examined, but all others which may happen to agree with them as they agree with each other. As our senses cannot possibly report to us all the contents of space, we cannot usually set any limits to the number of objects which may fall into any such class. At this point we begin to perceive the power and generality of thought which enables us at once to treat of indefinitely or even infinitely numerous objects. We can safely assert that whatever is true of any one object coming under a general notion or class is true of any of the other objects so far as they possess the common qualities implied in their belonging to the class. We must not place an individual thing in a class unless we are prepared to believe of it all that is believed of the class in general; but it remains as a matter of important consideration how far and in what manner we can safely undertake thus to assign the place of objects in that general system of classification which constitutes the whole body of science.

Twofold Meaning of General Names.

Etymologically the *meaning* of a name is what we are caused to think of when the name is used. Now every general name causes us to think of some one or more of the objects belonging to a class; it may also cause us to think of the common qualities possessed by those objects. A name is said to *denote* the distinct object of thought to which it may be applied; it *implies* at the same time the possession of certain qualities or circumstances. The number of objects denoted forms the *extent* of meaning of the term; the number of qualities implied forms the *intent* of meaning. Crystal is the name of any substance of which the molecules are arranged in a regular symmetrical manner. The substances or objects in question form the extent of meaning; the circumstance of having the molecules so arranged forms the intent of meaning.

When we compare a variety of general terms it may often be found that the meaning of one is included in the meaning of another. Thus all *crystals* are included among *material substances*, and all *opaque crystals* are included among *crystals:* here the inclusion is in extension. We may also have inclusion of meaning in regard to intension. For as all crystals are material substances, the qualities implied by the term material substance must be among those implied by crystal. Again, it is obvious that while in extension of meaning opaque crystals are but a part of crystals, *in intension* of meaning crystal is but part of opaque crystal. We increase the intent of meaning of a term by joining adjectives, or phrases equivalent to adjectives, to it, and the removal of such adjectives of course decreases the intensive meaning. Now concerning such changes of meaning the following all-important law holds universally true. *When the intent of meaning of a term is increased the extent is decreased;* and vice versâ, *when the extent is increased the intent is decreased.* In short, as one is increased the other is decreased.

This law refers only to logical changes. The number of steam engines in the world may be undergoing a rapid increase without the intensive meaning of the name being altered. The law will only be verified again when there is a real change in the intensive meaning, and an adjective may often be joined to a noun without making a change. *Elementary metal* is identical with *metal*; *mortal man* with *man*; it being a *property* of all metals to be elements, and all men to be mortals.

There is no limit to the amount of meaning which a term may have. A term may denote one object, or many, or an infinite number; it may imply a single quality, if such there be, or a group of any number of qualities, and yet the law connecting the extension and intension will infallibly apply. Taking the general name *planet*, we increase its intension and decrease its extension by prefixing the adjective *exterior*; and if we further add *nearest to the earth*, there remains but one planet *Mars*, to which the name can then be applied. Singular terms, which denote a single individual only, come under the same law of meaning as general names. They may be regarded as general names of which the meaning in extension is reduced to a minimum. Logicians have erroneously asserted, as it seems to me, that singular terms are devoid of meaning in intension, the fact being that they exceed all other terms in that kind of meaning, as I have elsewhere tried to show [a].

Abstract Terms.

Comparison of different objects, and analysis of the complex resemblances and differences which they present, lead us to the conception of *abstract qualities.* We learn to think of one object as not only different from another, but as differing in some particular point, such as colour, or weight, or size. We may then convert points of agreement or difference into separate objects of thought called *qualities*, and denoted by *abstract terms.* Thus the term *redness* means something in which a number of objects agree as to colour, and in virtue of which they are called red. Redness forms, in fact, the intensive meaning of the term red.

Abstract terms are strongly distinguished from general terms by possessing only one kind of meaning; for as they denote qualities there is nothing which they can in addition imply. The adjective 'red' is the name of red objects, but it implies the possession by them of the quality *redness*; but this latter term has one single meaning—the quality alone. Thus it arises that abstract terms are incapable of number or plurality. Red objects are numerically distinct each from each, and there are a multitude of such objects; but redness is a single existence which runs through all those objects, and is the same in one as it is in another. It is true that we may speak of *rednesses*, meaning different kinds or tints of redness, just as we may speak of *colours*, meaning different kinds of colours. But in distinguishing kinds, degrees, or other differences, we render the terms so far concrete. In that they are merely red there is but a single nature in red objects, and so far as things are merely coloured, colour is a single indivisible quality. Redness, so far as it is redness merely, is one and the same everywhere, and possesses absolute oneness or unity. In virtue of this unity we acquire the power of treating all instances of such quality as we may treat any one. We possess, in short, general knowledge.

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[a] See Mill, 'System of Logic,' Book I. chap. ii. section 5. Jevons' 'Elementary Lessons in Logic,' pp. 41–43; 'Pure Logic,' p. 6. See also Shedden's 'Elements of Logic,' *London, 1864, pp. 14, &c.*

Substantial Terms.

Logicians appear to have taken very little notice of a large class of terms which partake in certain respects of the character of abstract terms and yet are undoubtedly the names of concrete existing things. These terms are the names of substances, such as gold, carbonate of lime, nitrogen, &c. We cannot speak of two golds, twenty carbonates of lime, or a hundred nitrogens. There is no such distinction between the parts of a uniform substance as will allow of a discrimination of numerous individuals. The qualities of colour, lustre, malleability, density, &c., by which we recognise gold, extend through its substance irrespective of particular size or shape. So far as a substance is gold, it is one and the same everywhere; so that terms of this kind, which I propose to call *substantial terms*, possess the peculiar unity of abstract terms. Yet they are not abstract; for gold is of course a tangible visible body, entirely concrete, and existing physically independent of other bodies.

It is only when we break up, by actual mechanical division, the uniform whole which forms the meaning of a substantial term, that we introduce the notion of number. *Piece of gold* is a term capable of plurality; for there may be an endless variety of pieces discriminated from each other, either by their various shapes and sizes, or, in the absence of such marks, by occupying simultaneously different parts of space. In substance they are one: as regards the properties of space they are many. We need not further pursue this distinction between unity and plurality until we come to consider the principles of number in a subsequent chapter.

Collective Terms.

We must clearly distinguish between the *collective* and the *general meaning* of terms. The same name may be used to denote the whole body of existing objects of a certain kind, or any one of those objects taken separately. 'Man' may mean the aggregate of existing men, which we sometimes describe as *mankind*; it is also the general name applying to any man. The vegetable kingdom is the name of the whole aggregate of *plants*, but 'plant' itself is a general name applying to any one or other plant. Every material object may be conceived as divisible into parts, and is therefore collective as regards those parts. The animal body is made up of cells and fibres, a crystal of molecules; wherever physical division, or as it has been called *partition*, is possible, there we deal in reality with a collective whole. Thus the greater number of general terms are at the same time collective as regards each individual whole which they denote.

It need hardly be pointed out that we must not infer of a collective whole what we know of the parts, nor of the parts what we know only of the whole. The relation of whole and part is not one of identity, and does not allow of substitution. There may nevertheless be qualities or circumstances which are true alike of the whole and its parts. Thus a number of organ pipes tuned in unison produce an aggregate of sound *which is of* exactly the same pitch as each separate sound. In the case of substantial terms, certain qualities may be present equally in each minutest part as in the whole. The chemical nature of the largest mass of pure carbonate of lime in existence is the same as the nature of the smallest particle. In the case of abstract terms, again, we cannot draw a distinction between whole and part; what is true of redness in any case is always true of redness, so far as it is merely red.

Synthesis of Terms.

We continually combine simple terms together so as to form new terms of more complex meaning. Thus, to increase the intension of meaning of a term we write it with an adjective or a phrase of adjectival nature. By joining 'brittle' to 'metal,' we obtain a combined term, 'brittle metal,' which denotes a certain portion of the metals, namely such as are selected on account of possessing the quality of *brittleness.* As we have already seen, 'brittle metal' possesses less extension and greater intension than metal. Nouns, prepositional phrases, participial phrases and subordinate propositions may also be added to terms so as to increase their intension and decrease their extension.

In our symbolic language we need some mode of indicating this junction of terms, and the most convenient device will be the simple juxtaposition of the distinct letter-terms. Thus if A mean brittle, and B mean metal, then AB will mean brittle metal. Nor need there be any limit to the number of letters thus joined together, or the complexity of the notions which they may represent.

Thus if we take the letters

P = metal,
Q = white,
R = monovalent,

S = of specific gravity 10·5,
T = melting above 1000°C,
V = good conductor of heat and electricity,

then we can form a combined term PQRSTV, which will denote 'a white monovalent metal, of specific gravity 10·5, melting above 1000°C, and a good conductor of heat and electricity.'

There are many grammatical rules or usages concerning the junction of words and phrases to which we need pay no attention in logic. We can never say in ordinary language 'of wood table,' meaning 'a table of wood,' but we may consider 'of wood' as logically an exact equivalent of 'wooden'; so that if

X = of wood,
Y = table,

there is no reason why, in our symbols, XY should not be the correct term for 'table of wood.' In this case indeed we might substitute the corresponding adjective 'wooden,' but we should often fail to find any adjective answering exactly to a phrase. There is no single word which could express the notion 'of specific gravity 10·5': but logically we may consider these words as forming an adjective; and denoting this by S and metal by P, we may say that SP means 'metal of specific gravity 10·5.' It is one of many advantages in these blank letter-symbols that they enable us completely to abstract all grammatical peculiarities and fix our attention solely on the purely logical relations involved. Investigation will probably show that the rules of grammar are mainly founded upon traditional usage and have little logical signification. This indeed is sufficiently proved by the wide grammatical differences which exist between languages where the logical foundation must be the same.

Symbolic Expression of the Law of Contradiction.

The synthesis of terms is subject to the all-important Law of Thought, described in a previous section (p. 6) and called the Law of Contradiction. It is self-evident that no quality or circumstance can be both present and absent at the same time and place. This fundamental condition of all thought and all existence is expressed symbolically by a rule that a term and its negative shall never be allowed to come into combination. Such combined terms as Aa, Bb, Cc, &c. are self-contradictory and devoid of all meaning. If they represented anything, it would be what cannot exist, and cannot even be imagined in the mind. They can therefore only enter into our consideration to suffer immediate exclusion. The criterion of false reasoning, as we shall find, is that it involves self-contradiction, the affirming and denying of the same statement. Thus we might represent the object of all reasoning as the separation of the consistent and possible from the inconsistent and impossible; and we cannot make any inference without implying that certain combinations of terms are contradictory and excluded from thought. To conclude that 'all A's are B's' is equivalent to the assertion that 'A's which are not B's cannot exist.'

It will be convenient to have the means of indicating this exclusion of the self-contradictory; and we may use the familiar sign for *nothing*, the cipher o. Thus the second law of thought may be symbolised in the forms

$$Aa = 0 \qquad ABb = 0 \qquad ABCa = 0.$$

We may variously describe the meaning of o in logic as the *non-existent*, the *impossible*, the *self-inconsistent*, the *inconceivable*. Close analogy exists between this meaning and its mathematical signification.

Certain Special Conditions of Logical Symbols.

In order that we may argue and infer truly we must treat our logical symbols according to the fundamental laws of Identity and Difference. But in thus using our symbols we shall frequently meet with combinations of which the meaning will not at first be apparent. In some cases, for instance, we may learn that an object is 'yellow and round,' in other cases that it is 'round and yellow': there arises the question whether these two descriptions are identical in meaning or not. Or again, if we proved that an object was 'round round' the meaning of such an expression would be open to doubt. Accordingly we must take notice, before proceeding further, of certain special laws which govern the combination of logical terms.

In the first place the combination of a logical term with itself is without effect, just as the repetition of a statement does not alter the meaning of the statement: 'a round round object' is simply 'a round object.' What is yellow yellow is merely yellow; metallic metals cannot differ from metals, nor elementary elements from elements. In our symbolic language we may similarly hold that AA is identical with A, or

$$A = AA = AAA = \&c.$$

The late Professor Boole is the only logician in modern times who has drawn attention to this remarkable property of logical terms[b]; but in place of the name which he gave to the law, I have proposed to call it The Law of Simplicity[c]. Its high importance will only become apparent when we attempt to determine the relations of logical and mathematical science. Two symbols of quantity, and only two, seem to obey this law; we may say that $1.1 = 1$, and $0.0 = 0$ (taking 0 to mean absolute zero or $1 - 1$); there is apparently no other number which combined with itself gives an unchanged result. I shall point out, however, in the chapter upon Number, that in reality all numerical symbols obey this logical principle.

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[b] 'Mathematical Analysis of Logic,' Cambridge, 1847, p. 17. 'An Investigation of the Laws of Thought,' London, 1854, p. 29.

[c] 'Pure Logic,' p. 15.

It is curious that this Law of Simplicity, though almost unnoticed in modern times, was known to Boëthius, who makes a singular remark in his treatise 'De Trinitate et Unitate Dei' (p. 959). He says, 'If I should say sun, sun, sun, I should not have made three suns, but I should have named one sun so many times[d].' Ancient discussions concerning the doctrine of the Trinity drew more attention to subtle questions concerning the nature of unity and plurality than has ever since been given to them.

It is a second law of logical symbols that order of combination is a matter of indifference. 'Rich and rare gems' are the same as 'rare and rich gems,' or even as 'gems, rich and rare.' Grammatical, rhetorical or poetic usage may give considerable significance to order of expression. The limited power of our minds prevents our grasping many ideas at once, and thus the order of statement may produce some effect, but not in a strictly logical manner. All life proceeds in the succession of time, and we are obliged to write, speak, or even think of things and their qualities one after the other; but between the things and their qualities there need be no such relation of order in time or space. The sweetness of sugar is neither before nor after its weight and solubility. The hardness of a metal, its colour, weight, opacity, malleability, electric and chemical properties, are all coexistent and coextensive, pervading the metal and every part of it in perfect community, none before nor after the others. In our words and symbols we cannot observe this natural condition; we must name one quality first and another second, just as some one must be the first to sign a petition, or to walk foremost in a procession. In nature there is no such precedence.

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[d] 'Velut si dicam Sol, Sol, Sol, non tres soles effecerim, sed uno toties prædicaverim.'

A little reflection will show that knowledge in the highest perfection would consist in the *simultaneous* possession of a multitude of facts. To comprehend a science perfectly we should have every fact present with every other fact. We must write a book and we must read it successively word by word, but how infinitely higher would be our powers of thought if we could grasp the whole in one collective act of consciousness. Compared with the brutes we do possess some slight approximation to such power, and it is just conceivable that in the indefinite future mind may acquire a vast increase of capacity, and be less restricted to the piecemeal examination of a subject. But I wish here to make plain that there is no logical foundation for the successive character of thought and reasoning unavoidable under our present mental conditions. The fact that we must think of one thing first, and another second, is a logical weakness and imperfection. We must describe metal as 'hard and opaque,' or 'opaque and hard,' but in the metal itself there is no such difference of order; the properties are simultaneous and coextensive in existence.

Setting aside all grammatical peculiarities which render a substantive less moveable than an adjective, and disregarding any meaning indicated by emphasis or marked order of words, we may state, as a general law of logic, that AB is identical with BA.

$$AB = BA$$

$$ABC = ACB = BCA = \&c.$$

The late Professor Boole first drew attention, so far as I know, to this property of logical terms, and he called it the property of Commutativeness[e]. He not only stated the law with the utmost clearness, but pointed out that it is a Law of Thought rather than a Law of Things. I shall have in various parts of the following pages to show how the necessary imperfection of our symbols expressed in this law clings to our modes of expression, and introduces complication into the whole body of mathematical formulæ, which are really founded on a logical basis.

It is of course apparent that the power of commutation belongs only to terms related in the simple logical mode of synthesis. No one can confuse 'a house of bricks,' with 'bricks of a house,' 'twelve square feet' with 'twelve feet square,' 'the water of crystallization' with 'the crystallization of water.' All relations which involve differences of time and space are inconvertible; the higher must not be made to change place with the lower, or the first with the last. For the parties concerned there is all the difference in the world between A killing B and B killing A. The law of commutativeness simply asserts that difference of order does not attach to the connection between the properties and circumstances of a thing—to what I shall call *simple logical relations.*

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[e] 'Laws of Thought,' p. 29.

CHAPTER III.

PROPOSITIONS.

We now proceed to consider the variety of forms of propositions in which the truths of science must be expressed. I shall endeavour to show that, however diverse these forms may be, they all admit the application of the one same principle of influence, that what is true of one thing or circumstance is true of the like or same. This principle holds true whatever be the kind or manner of the likeness, provided proper regard be had to its degree. Propositions may assert an identity of time, space, manner, quantity, degree, or any other circumstance in which things may agree or differ.

We find an instance of a proposition concerning time in the following :—'The year in which Newton was born, was the year in which Galileo died.' This proposition expresses an approximate identity of time between two events: hence whatever is true of the year in which Galileo died is true of that in which Newton was born, and *vice versâ*. 'Tower Hill is the place where Raleigh was executed' expresses an identity of place; and whatever is true of the one spot is true of the spot otherwise defined, but in reality the same. In ordinary language we have many propositions obscurely expressing identities of number, quantity, or degree. 'So many men, so many minds,' is a proposition concerning number or an equation: whatever is true of the number of men is true of the number of minds, and *vice versâ*. 'The density of Mars is (nearly) *the same as that of the* Earth,' 'The force of gravity is directly as the product of the masses, and inversely as the square of the distance,' are propositions concerning magnitude or degree. Logicians have not paid adequate attention to the great variety of propositions which can be stated by the use of the little conjunction *as*, together with *so*. 'As the home so the people,' is a proposition expressing identity of manner; and a great number of similar propositions all indicating some kind of resemblance might be quoted. Whatever be the special kind or form of identity, all such expressions of identity are subject to the great principle of inference; but as we shall in later parts of this work treat more particularly of inference in cases of number and magnitude, we will here confine our attention to the logical propositions which involve only notions of quality.

Simple Identities.

The most important class of propositions consists of those which fall under the formula

$$A = B,$$

and may be called *simple identities*. I may instance, in the first place, those most elementary propositions which express the exact similarity of a quality encountered in two or more objects. I may compare by memory or otherwise the colour of the Pacific ocean with that of the Atlantic, and declare them identical. I may assert that 'the smell of a rotten egg is that of hydrogen sulphide,' 'the taste of silver hyposulphite is that of cane sugar,' 'the sound of an earthquake is that of distant artillery.' Such are propositions stating, accurately or otherwise, the identity or non-identity of simple physical sensations. Judgments of this kind are necessarily presupposed in more complex judgments. If I declare that '*this coin is made of gold*,' I must base the judgment upon the exact likeness of the substance in several qualities to other pieces of substance which are undoubtedly of gold. I must make a judgment of the colour, the specific gravity, the hardness, sound, and chemical properties; and each of these judgments might be expressed in an elementary proposition, 'the colour of this coin is the colour of gold,' and so on. Even when we establish the identity of a thing with itself under a different name or aspect, it is by distinct judgments concerning single circumstances. To prove that the Homeric χαλκός is copper we must show the identity of each quality recorded of χαλκός with a quality of copper. To establish Deal as the landing-place of Cæsar, every circumstance must be shown to agree. If the modern Wroxeter is the ancient Uricon̄ium, there must be the like agreement of all features of the country not subject to alteration by time.

All such identities may be expressed in the form $A = B$. We may say

Colour of Pacific Ocean = Colour of Atlantic Ocean.
Smell of rotten egg = Smell of hydrogen sulphide.

In these and similar propositions we assert identity of single qualities or sensations. But in the same form we may express identity of any group of qualities, as in

χαλκός = Copper.
Deal = Landing-place of Cæsar.

A multitude of propositions involving singular terms fall into the same form, as in

The Pole star = The slowest-moving star.
Jupiter = The greatest of the planets.
The ringed planet = The planet having seven satellites.
The Queen of England = The Queen of India.
The number two = The even prime number.
Honesty = The *best policy.*

In mathematical and scientific theories we often meet with simple identities capable of expression in the same form. Thus in mechanical science 'The process for finding the resultant of forces = the process for finding the resultant of simultaneous velocities[a].' Theorems in geometry often give results in this form, as—

Equilateral triangles = Equiangular triangles.
Circle = Finite plane curve of constant curvature.
Circle = Curve of least perimeter.

The more profound and important laws of nature are often expressible in the form of identities; in addition to some instances which have already been given I may suggest—

Crystals of cubical system = Crystals incapable of double refraction.

All definitions are necessarily of this form of simple identity, whether the objects defined be many, few, or singular. Thus we may say—

Common salt = Sodium chloride.
Chlorophyl = Green colouring matter of leaves.
Square = Equal-sided rectangle.

It is an extraordinary fact that propositions of this elementary form, all-important and very numerous as they are, had no recognised place in Aristotle's system of Logic. Accordingly their importance was overlooked until very recent times, and logic was the most deformed of sciences. But it is quite impossible that Aristotle or any other person should avoid constantly using them; not a term could be defined without their use. In one place at least Aristotle actually notices a proposition of the kind. He observes:—'We sometimes say that that white thing is Socrates, or that the object approaching is Callias[b].' Here we certainly have simple identity of terms; but he considered such propositions purely accidental, and came to the extraordinary conclusion, that 'Singulars cannot be predicated of other terms.'

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[a] Thomson and Tait, 'Treatise on Natural Philosophy,' vol. i. p. 182.
[b] *'Prior Analytics,' I. cap.* xxvii. 3.

Propositions may also express the identity of extensive groups of objects taken collectively or in one connected whole; as when we say—

'The Queen, Lords, and Commons = The Legislature of the United Kingdom.'

When Blackstone asserts, 'The only true and natural foundation of society are the wants and fears of individuals,' we must interpret him as meaning that the whole of the wants and fears of individuals in the aggregate form the foundation of society. But many propositions which might seem to be collective are but groups of singular propositions or identities. When we say 'Potassium and sodium are the metallic bases of potash and soda,' we obviously mean—

Potassium = Metallic base of potash;
Sodium = Metallic base of soda.

It is the work of grammatical analysis to separate the various propositions often combined in a single sentence. Logic cannot be properly required to interpret the forms and devices of language, but to treat the meaning or information when clearly exhibited.

Partial Identities.

However numerous and important may be propositions expressing *simple identity* of one term or class with another, there is an almost equally important kind of proposition which I propose to call *a partial identity.* When we say that 'All mammalia are vertebrata,' we do not mean that mammalian animals are identical with vertebrate animals, but only that the mammalian form a *part of the class vertebrata.* Such a proposition was regarded in the old logic as asserting the inclusion of one class in another, or of an object in a class. It was called a universal affirmative proposition, because the attribute *vertebrate* was affirmed of the whole subject *mammalia;* but the attribute was said to be *undistributed*, because not all vertebrata were of necessity involved in the proposition. Aristotle, overlooking the importance of simple identities, and indeed almost denying their existence, unfortunately founded his system upon the notion of inclusion in a class, in place of identity. He regarded inference as resting upon the rule that what is true of the containing class is true of the contained, instead of the vastly more general rule that what is true of a class or thing is true of the like. Thus he not only reduced logic to a fragment of its proper self, but destroyed the deep analogies which bind together logical and mathematical reasoning. Hence a crowd of defects, difficulties and errors which will long disfigure the first and simplest of the sciences.

It is surely evident that the relation of inclusion rests upon a relation of identity. Mammalian animals cannot be included among vertebrates unless they be identical with part of the vertebrates. Cabinet Ministers are included almost always in the class Members of Parliament, because they are identical with some who sit in Parliament. We may indicate this identity with a part of the larger class in various ways; as for instance—

Mammalia = part of the vertebrata
Diatoms = species of plants.
Cabinet Ministers = some Members of Parliament.
Iron = a metal.

In ordinary language the verbs *is* or *are* express mere inclusion more often than not. *Men are mortals,* means that *men* form a part of the class *mortal,* but great confusion exists between this sense of the verb and that in which it expresses identity, as in 'The sun is the centre of the planetary system.' The introduction of the indefinite article *a* often seems to express partiality, as when we say 'Iron is a metal' we clearly mean *one only* of several metals.

Certain eminent recent logicians have proposed to avoid the indefiniteness in question by what is called the Quantification of the Predicate, and they have generally used the little word *some* to show that only a part of the predicate is identical with the subject[c]. *Some* is an *indeterminate adjective;* it implies unknown qualities by which we might select the part in question if they were known, but it gives no hint as to their nature. I might make extensive use of such an indeterminate sign to express partial identities in this work. Thus, taking the special symbol V = some, the general form of a partial identity would be A = VB, and in Boole's Logic expressions of the kind were freely used. But I find that indeterminate symbols only introduce complexity, and destroy the beauty and simple universality of the system which may be created without their use. A vague word like *some* is only used in ordinary language by *ellipsis*, and to avoid the trouble of attaining accuracy. We can always substitute for it more definite expressions if we like; but when once the indefinite *some* is introduced we cannot replace it by the special description. We do not know whether *some* colour is red, yellow, blue, or what it is; but on the other hand *red* colour is certainly *some* colour, as is also yellow, blue, &c.

Throughout this system of logic I shall usually dispense with all such indefinite expressions; and this can readily be done by substituting one of the other terms. To express the proposition 'All A's are some B's' I shall not use the form A = VB, but

$$A = AB.$$

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[c] 'Elementary Lessons in Logic,' p. 183. 'Substitution of Similars,'

This formula expresses that the class A is identical with the class AB; and as the latter must be a part at least of the class B, it implies the inclusion of the class A in that of B. Thus we might represent our former example thus—

Mammalia = Mammalian vertebrata.

This proposition asserts identity between a part of the vertebrata and the mammalia. If it is asked What part? the proposition affords no answer except that it is the part which is mammalian; but the assertion 'mammalia = some vertebrata' tells us no more.

It is quite likely that some readers may think this mode of representing the universal affirmative proposition of the old logic artificial and complicated. I will not undertake to convince them of the opposite at this point of the system. My justification for it will be found, not in the immediate treatment of this proposition, but in the general harmony which it enables us to discover between all parts of reasoning. I have no doubt that this is the point of critical difficulty in the relation of logical to other forms of reasoning. Grant this mode of denoting that 'all A's are B's,' and I fear no further difficulties; refuse it, and we find want of analogy and endless complication in every direction. For instance —Aristotle, in accepting inclusion of class in class as the fundamental relation of logic, was at once obliged to ignore the existence of the very extensive and all-important class of propositions denoting the similarity of one thing with another. It is on general grounds that I hope to show overwhelming reasons for seeking to reduce every kind of proposition to the form of an identity.

I may add that not a few previous logicians have accepted this view of the universal affirmative proposition. Boole often employed this mode of expression, and

Spalding[d] distinctly says that the proposition 'all metals are minerals' might be described as an assertion of *partial identity* between the two classes. Hence the name which I have adopted for the proposition.

Limited Identities.

A highly important class of propositions have the general form

$$AB = AC,$$

expressing the identity of the class AB with the class AC. In other words, 'Within the sphere of the class of things A all the B's are all the C's,' or 'The B's and C's, which are A's, are identical.' But it will be observed that nothing is asserted concerning things which are outside of the class A; and thus the identity is of limited extent. It is the proposition B = C limited to the sphere of the class A. Thus if we say 'Plants are devoid of locomotive power,' we must limit the statement to large plants, since minute microscopic plants often have very remarkable powers of motion. When we say 'Metals possess metallic lustre,' we mean in their uncombined state.

A barrister may make numbers of most general statements concerning the relations of persons and things in the course of an argument, but it is of course to be understood that he speaks only of persons and things under the English Law. Even mathematicians make statements which are not true with absolute generality. They say that imaginary roots enter into equations by pairs; but this is only true under the tacit condition that the equations in question shall not have imaginary coefficients.[e]

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[d] 'Encyclopædia Britannica,' Eighth Ed. art. Logic, sect. 37, note. Reprint, p. 79.

[e] De Morgan 'On the Root of any Function.' Cambridge Philosophical Transactions, 1867, vol. xi. p. 25.

The universe, in short, within which they habitually discourse, is that of equations with real coefficients. These implied limitations form part of that great mass of tacit knowledge which accompanies all special arguments.

It is worthy of inquiry whether almost all identities are not really limited to an implied sphere of meaning. When we make such a plain statement as 'Gold is malleable' we obviously speak of gold only in its solid state; when we say that 'Mercury is a liquid metal' we must be understood to exclude the frozen condition to which it may be reduced in the Arctic regions. Even when we take such a fundamental law of nature as 'All substances gravitate,' we must mean by substance, material substance, not including that basis of heat, light and electrical undulations which occupies space and possesses many mechanical properties, but not gravity. The proposition then is really of the form

Material substance = Material gravitating substance.

To De Morgan is due the remark, that we do usually think and argue in a limited universe or sphere of notions even when it is not expressly stated[f].

Negative Propositions.

In every act of intellect, as we have seen, we are engaged with a certain degree of identity or difference between certain things or sensations compared together. Hitherto I have treated only of identities; and yet it might seem that the relation of difference must be infinitely more common than that of likeness. One thing may resemble a great many other things, but then it differs from all remaining things in the world. Difference or diversity may almost be said to constitute life, being to thought what motion is to a river. The very perception of an object involves its discrimination from all other objects. But we may nevertheless be said to detect resemblance as often as we detect difference. We cannot, in fact, assert the existence of a difference, without at the same time implying the existence of an agreement.

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[f] 'Syllabus of a Proposed System of Logic,' §§ 122, 123.

If I compare mercury, for instance, with other metals, and decide that it is *not solid*, here is a difference between mercury and solid things, expressed in a negative proposition; but there must be implied, at the same time, an agreement between mercury and the other substances which are not solid. As it is impossible in the alphabet to separate the vowels from the consonants without at the same time separating the consonants from the vowels, so I cannot select as the object of thought *solid things*, without thereby throwing together into another class all things which are *not solid*. The very fact of not possessing a quality, constitutes a new quality or circumstance which may equally be the ground of judgment and classification. In this point of view, agreement and difference are ever the two sides of the same act of intellect, and it becomes equally possible to express the same judgment in one or other aspect.

Between affirmation and negation there is accordingly a perfect balance or equilibrium. Every affirmative proposition implies a negative one, and *vice versâ*. It is even a matter of indifference, in a logical point of view, whether a positive or negative term be used to denote a given quality and the class of things possessing it. If the ordinary state of man's body be called *good health*, then in other circumstances he is said *not to be in good health*; but we might equally describe him in the latter state as *sickly*, and in his normal condition he would be *not sickly*. Animal and vegetable substances are now called *organic*, so that the other substances, forming an immensely greater part of the globe, are described negatively as *inorganic.* But we might, with at least equal logical correctness, have described the preponderating class of substances as *mineral,* and then vegetable and animal substances would have been *non-mineral.*

It is plain that any positive term, and its corresponding negative divide between them the whole universe of thought: whatever does not fall into one must fall into the other, by the third fundamental Law of Thought, the Law of Duality. It follows at once that there are two modes of representing a difference. Suppose that the things or classes represented by A and B are found to differ, we may indicate the result of the judgment by the notation (see p. 20)

$$A \sim B.$$

But we may now represent the same judgment by the assertion that A agrees with those things which differ from B, or that A agrees with the not-B's. Using our notation for negative terms (see p. 17), we obtain

$$A = Ab$$

as the expression of the ordinary negative proposition. Thus if we take A to mean quicksilver, and B solid, then we have the following proposition:—

Quicksilver = Quicksilver not-solid.

There may also be several other classes of negative propositions, of which no notice was taken in the old logic. We may have cases where all A's are not-B's, and at the same time all not-B's are A's; there may, in short, be a simple identity between A and not-B, which may be expressed in the form

$$A = b.$$

An example of this form would be

Conductors of electricity = non-electrics.

We shall also frequently have to deal as results of deduction, with simple, partial, or limited identities between negative terms, in the forms

$$a = b, \quad a = ab, \quad aC = bC.$$

It would be equally possible to represent affirmative propositions in the negative form. Thus 'Iron is solid,' might be expressed as 'Iron is not not-solid,' or 'Iron is not fluid'; or, taking A and *b* for the terms 'iron,' and 'not-solid,' the form would be

$$A \sim b.$$

But there are very strong reasons why we should employ all propositions in their affirmative form. All inference proceeds by the substitution of equivalents, and a proposition expressed in the form of an identity is ready to yield all its consequences in the most direct manner. As will be more fully shown, we can infer *in* a negative proposition, but not *by* it. Difference is incapable of becoming the ground of inference; it is only the implied agreement with other differing objects, which admits of deduction: and it will always be found advantageous to employ propositions in the form which exhibits clearly all the implied agreements.

Conversion of Propositions.

The old books of logic contain many rules concerning the conversion of propositions, that is, the transposition of the subject and predicate in such a way as to obtain a new proposition which will be equally true with the original. The reduction of every proposition to the form of an identity renders all such rules and processes useless. Identity is essentially reciprocal. If the colour of the Atlantic Ocean is the same as that of the Pacific Ocean, that of the Pacific must be the same as that of the Atlantic. Sodium chloride being identical with common salt, common salt must be identical with sodium chloride. If the number of windows in Salisbury Cathedral equals the number of days in the year, the number of days in the year must equal the number of the windows. Lord Chesterfield was not wrong when he said, 'I will give anybody their choice of these two truths, which amount to the same thing; He who loves himself best is the honestest man; or, The honestest man loves himself best[g].' Scotus Erigena exactly expresses this reciprocal character of identity in saying, 'There are not two studies, one of philosophy and the other of religion; true philosophy is true religion, and true religion is true philosophy.'

A mathematician would not think it worth mention that if $x=y$ then also $y=x$. He would not consider these to be two equations at all, but one same equation accidentally written in two different manners. In written symbols one of two names must come first, and the other second, and a like succession must perhaps be observed in our thoughts: but in the relation of identity there is no need for succession in order; each is simultaneously equal and identical to the other. These remarks will hold true equally of logical and mathematical identity; so that I shall consider the two forms

$$A=B \text{ and } B=A$$

to express exactly the same identity differently written. All need for rules of conversion disappears, and there will be no single proposition in the system which may not be written with either term foremost. Thus $A=AB$ is the same as $AB=A$, $AB=AC$ as $AC=AB$, and so on.

The same remarks are partially true of differences or inequalities, which are also reciprocal to the extent that one thing cannot differ from a second without the second differing from the first. Mars differs in colour from

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[g] Chesterfield's Letters, 8vo, 1744; vol. i. p. 302.

Venus, and Venus must differ from Mars. The Earth differs from Jupiter in density; therefore Jupiter must differ from the Earth. Speaking generally, if $A \sim B$ we shall also have $B \sim A$, and these two forms may be considered expressions of the same difference. But the reader will notice that the relation of differing things is not wholly reciprocal. The density of Jupiter does not differ from that of the Earth in the same way that that of the Earth differs from that of Jupiter. The change of sensation which we experience in passing from Venus to Mars is not the same as what we experience in passing back to Venus, but just the opposite in nature. The colour of the sky is lighter than that of the ocean; therefore that of the ocean cannot be lighter than that of the sky, but darker. In these and all similar cases we gain a notion of *direction* or character of change, and results of immense importance may be shown to rest on this notion. For the present we shall be concerned with the mere fact of identity existing or not existing.

Twofold Interpretation of Propositions.

Terms, as we have seen (p. 31), may have a meaning either in extension or intension; and according as one or the other meaning is attributed to the terms of a proposition, so may a different interpretation be assigned to the proposition itself. When the terms are abstract we must read them in intension, and a proposition connecting such terms must denote the identity or non-identity of the qualities respectively denoted by the terms. Thus if we say

Equality = Identity of magnitude,

the assertion means that the circumstance of being equal exactly corresponds with the circumstance of being identical in magnitude. Similarly in

Opacity = Incapability of transmitting light,

the quality of being incapable of transmitting light is declared to be the same as the intended meaning of the word opacity.

When general names form the terms of a proposition we may apply a double interpretation. Thus

Exogens = Dicotyledons

means either that the qualities which belong to all exogens are the same as those which belong to all dicotyledons, or else that every individual falling under one name falls equally under the other. Hence it may be said that there are two distinct fields of logical thought. We may argue either by the qualitative meaning of names or by the quantitative, that is, the extensive meaning. Every argument involving concrete plural terms might be converted into one involving only abstract singular terms, and *vice versâ*. But there are many reasons for believing that the intensive or qualitative form of reasoning is the primary and fundamental one. It is sufficient to point out that we may use abstract terms which contain no reference to an extensive meaning; and when there is a mode which we must sometimes and may always adopt, it is higher in importance than a mode which we never need adopt necessarily.

CHAPTER IV.

DEDUCTIVE REASONING.

THE general principle of inference having been explained in the previous chapters, and a suitable system of symbols provided, we have now before us the comparatively easy task of tracing out the most common and important forms of deductive reasoning. The general problem of deduction is as follows:—*From one or more propositions called premises to draw such other propositions as will necessarily be true when the premises are true.* By deduction we investigate and unfold the information contained in the premises; and this we can do by one single rule—*For any term occurring in any proposition or expression substitute the expression which is asserted in any premise to be identical with it.* To obtain certain deductions, especially those involving negative conclusions, we shall require to bring into use the second and third Laws of Thought, and the process of reasoning will then be called *Indirect Deduction.* In the present chapter, however, I shall confine my attention to those results which can be obtained by the process of *Direct Deduction*, that is, by applying to the premises themselves the rule of substitution. It will be found that we can combine in one harmonious system, not only the various moods of the ancient syllogism, but a great number of equally important forms of reasoning, which had no distinct place in the old logic. We can at the same time dispense entirely with the elaborate apparatus of logical rules and mnemonic lines, which were requisite so long as the vital principle of reasoning was not clearly expressed.

Immediate Inference.

Probably the simplest of all forms of inference is that which has been called *Immediate Inference*, because it can be performed upon a single proposition. It consists in joining an adjective, or other qualifying clause of the same nature, to both sides of an identity, and asserting the equivalence of the terms thus produced. For instance, since

Conductors of electricity = Non-electrics,

it follows that

Liquid conductors of electricity = Liquid non-electrics.

If we suppose that

Plants = Bodies decomposing carbonic acid,

it follows that

Microscopic plants = Microscopic bodies decomposing carbonic acid.

In general symbols, from the identity

$$A = B$$

we can infer the identity

$$AC = BC.$$

This is but a case of plain substitution; for by the first Law of Thought it must be admitted that

$$AC = AC,$$

and if in the second side of this identity we substitute for A its equivalent B, we obtain

$$AC = BC.$$

In like manner from the partial identity

$$A = AB$$

we may obtain

$$AC = ABC$$

by an exactly similar form of substitution; and in every other case the rule will be found capable of verification by the principle of inference. The process when performed as here described will be found free from the liability to error which I have shown [a] to exist in Immediate Inference by added Determinants, as described by Dr. Thomson [b].

Inference with Two Simple Identities.

One of the most common forms of inference, and one to which I shall especially direct attention, is practised with two simple identities. From the two statements that 'London is the capital of England' and 'London is the most populous city in the world,' we instantaneously draw the conclusion that 'The capital of England is the most populous city in the world.' Similarly, from the identities

Hydrogen = Substance of least density
Hydrogen = Substance of least atomic weight,

we infer

Substance of least density = Substance of least atomic weight.

The general form of the argument is exhibited in the symbols

$$B = A \qquad (1)$$
$$B = C \qquad (2)$$
$$\text{hence} \qquad A = C. \qquad (3)$$

We may describe the result by saying that terms identical with the same term are identical with each other; and it is impossible to overlook the analogy to the first axiom of Euclid that 'things equal to the same thing are equal to each other.' It has been very commonly supposed that this was a fundamental principle of thought incapable of reduction to anything simpler. But I entertain no doubt that this form of reasoning is only one case of the general rule of inference. We have two propositions, $A = B$ and $B = C$, and we may for a moment consider the second one as affirming a truth concerning B while the former one informs us that B is identical with A; hence by substitution we may affirm the same truth of A. It happens in this particular form that the truth affirmed is identity to C, and we might, if we had preferred, have considered the substitution as made by means of the second identity in the first. Having two identities we have a choice of the mode in which we will make the substitution, though the result is exactly the same in either case.

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[a] 'Elementary Lessons in Logic,' p. 86.

[b] 'Outline of the Laws of Thought,' § 87.

Now compare the three following formulæ

(1) $A = B = C$ hence $A = C$
(2) $A = B \sim C$ hence $A \sim C$
(3) $A \sim B \sim C$, no inference.

In the second formula we have an identity and a difference, and we are able to infer a difference; in the third we have two differences and are unable to make any inference at all. Because A and C both differ from B, we cannot tell whether they will or will not differ from each other. The flowers and leaves of a plant may both differ in colour from the earth in which the plant grows, and yet they may differ from each other; in other cases the leaves and stem may both differ from the soil and yet agree with each other. Where we have difference only we can make no inference; where we have identity we can infer. This fact gives great countenance to my assertion that inference proceeds always through an identity, but may be indifferently effected in a difference or an identity.

Deferring a more complete discussion of this point, I will only mention now that arguments from double identity occur very frequently, and are usually taken for granted owing to their extreme simplicity. In the equivalency of words it must be constantly employed. If the ancient Greek χαλκός is our *copper*, then it must be the French *cuivre*, the German *kupfer*, the Latin *cuprum*, because these are words, in one sense at least, equivalent to copper. Whenever we can give two definitions or expressions for the same term, the formula applies; thus Senior defined wealth as 'whatever is transferable, limited in supply, and productive of pleasure or preventive of pain;' it is also equivalent to 'whatever has value in exchange;' hence obviously 'Whatever has value in exchange' = 'Whatever is transferable, limited in supply, and productive of pleasure or preventive of pain.' Two expressions for the same term are often given in the same sentence, and their equivalency implied. Thus Thomson and Tait say[c], 'The naturalist may be content to know matter as that which can be perceived by the senses, or as that which can be acted upon by or can exert force.' I take this to mean—

Matter = what can be perceived by the senses;
Matter = what can be acted upon by or can exert force.

For the term 'matter' in either of these identities we may substitute its equivalent given in the other definition. Elsewhere they often employ sentences of the form exemplified in the following[d]; 'The integral curvature, or whole change of direction of an arc of a plane curve, is the angle through which the tangent has turned as we pass from one extremity to the other.' This sentence is certainly of the form—

The integral curvature = the whole change of direction, &c. = the angle through which the tangent has turned, &c.

Disguised cases of the same kind of inference occur throughout all sciences, and a remarkable instance is found in algebraic geometry. Mathematicians readily show that every equation of the form $y = mx + c$ is equivalent to or represented by a straight line; it is also easily proved that the same equation is equivalent to one of the form $Ax + By + C = 0$, and *vice versâ*. Hence it follows that every equation of the first degree is equivalent to or represents a straight line[e].

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[c] 'Treatise on Natural Philosophy,' vol. i. p. 161.

[d] *Ibid.* vol. i. p. 6.

Inference with a Simple and a Partial Identity.

A form of reasoning somewhat different from that last considered consists in inference between a simple and a partial identity. If we have two propositions of the form

$$A = B,$$
$$B = BC,$$

we may then substitute for B in either proposition its equivalent in the other, getting in both cases $A = BC$; in this we may if we like make a second substitution for B, getting

$$A = AC.$$

Thus, since 'Mont Blanc is the highest mountain in Europe, and Mont Blanc is deeply covered with snow,' we infer by an obvious substitution that 'The highest mountain in Europe is deeply covered with snow.' These propositions when rigorously stated fall into the form above exhibited.

This form of inference is constantly employed when for a term we substitute its definition, or *vice versâ*. The very purpose of a definition is in fact to allow a single term to be employed in place of a long descriptive phrase. Thus when we say 'Circles are curves of the second degree,' we may substitute the definition of a circle, getting 'A plane curve, all points of whose perimeter are at equal distances from a certain fixed point, is a curve of the propositions are of the form

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[e] Todhunter's 'Plane Co-ordinate Geometry,' chap. ii. pp. 11–14.

$$A = AB$$
$$B = c;$$

whence, by substitution,

$$A = Ac.$$

Inference of a Partial from Two Partial Identities.

However common be the cases of inference already noticed, there is a form occurring almost more frequently, and which deserves much attention because it occupied a prominent place in the ancient syllogistic system. That system strangely overlooked all the kinds of argument we have as yet considered, and selected as the type of all reasoning one which employs two partial identities as premises. Thus from the propositions

Sodium is a metal (1)
Metals conduct electricity, (2)

we may conclude that

Sodium conducts electricity. (3)

Taking A, B, C, respectively to represent the three terms, the premises are of the form

$$A = AB \qquad (1)$$
$$B = BC. \qquad (2)$$

Now for B in (1) we can substitute its description as given in (2), obtaining

$$A = ABC, \qquad (3)$$

or, in words, from

Sodium = sodium metal (1)
Metal = metal conducting electricity, (2)

we infer

Sodium = sodium metal conducting electricity, (3)

which in the elliptical language of common life becomes

‘Sodium conducts electricity.’

The above is a syllogism in the mood called Barbara[f] in the truly barbarous language of ancient logicians; and the first figure of the syllogism alone contained three other moods which were esteemed distinct forms of argument. But it is worthy of notice that without any real change in our form of inference we readily include these three other moods under it. The negative mood Celarent will be represented by the example

Neptune is a planet (1)
No planet has retrograde motion, (2)
hence Neptune has not retrograde motion. (3)

If we put A for Neptune, B for planet, and C for 'having retrograde motion,' then by the corresponding negative term c, we denote 'not having retrograde motion.' The premises now fall into the form

$$A = AB \qquad (1)$$
$$B = Bc, \qquad (2)$$

and by substitution for B, exactly as before, we obtain

$$A = ABc. \qquad (3)$$

What is called in the old logic a particular conclusion may be deduced without any real variation in the symbols. Particular quantity is indicated, as before mentioned (p. 49), by joining to the term an indefinite adjective of quantity, such as *some*, *a part*, *certain*, &c., meaning that an unknown part of the term enters into the proposition as subject. Considerable doubt and ambiguity arise out of the question whether the part may not in some cases be the whole, and in the syllogism at least it must be understood in this sense[g]. Now if we take a letter to represent this indefinite part, we need make no change in our formulæ to express either of the syllogisms Darii or Ferio. Consider the example—

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[f] An explanation of this and other technical terms of the old logic will be found in my 'Elementary Lessons in Logic,' Second Ed. 1871. Macmillan & Co.

[g] 'Elementary Lessons in Logic,' pp. 67, 79.

Some metals are of less density than water (1)
All bodies of less density than water will float upon its surface (2)
Some metals will float upon its surface. (3)

Let A = some metals
B = body of less density than water
C = floating on the surface of water;

then the propositions are evidently as before,

$$A = AB \quad (1)$$
$$B = BC; \quad (2)$$

hence

$$A = ABC. \quad (3)$$

Thus the syllogism Darii does not really differ from Barbara. If the reader prefer it, we can readily employ a distinct symbol for the indefinite sign of quantity.

Let P = some
Q = metal,

B and C having the same meanings as before. Then the premises become

$$PQ = PQB \quad (1)$$
$$B = BC; \quad (2)$$

hence, by substitution, as before,

$$PQ = PQBC. \quad (3)$$

Except that the formulæ look a little more complicated there is no difference whatever.

The mood Ferio is of exactly the same character as Darii or Barbara, except that it involves the use of a negative term. Take the example—

Bodies which are equally elastic in all directions do not doubly refract light,
Some crystals are bodies equally elastic in all directions; therefore some crystals do not doubly refract light.

Assigning the letters as follows—

A = some crystals.
B = bodies equally elastic in all directions
C = doubly refracting light
c = not doubly refracting light.

Our argument is of the same form as before, and may be concisely stated in one line

$$A = AB = ABc.$$

If we take PQ for the indefinite *some crystals*, we have

$$PQ = PQB = PQBc.$$

The only difference is that the negative term c occurs instead of C in the mood Darii (p. 68).

On the Ellipsis of Terms in Partial Identities.

The reader will probably have noticed that the conclusion which we obtain from premises is often more full than that drawn by the old Aristotelian processes. Thus from 'Sodium is a metal,' and 'Metals conduct electricity,' we inferred (p. 66) that 'Sodium = sodium metal, conducting electricity,' whereas the old logic simply concludes that 'Sodium conducts electricity.' Symbolically, from $A = AB$, and $B = BC$, we get $A = ABC$, whereas the old logic gets at the most $A = AC$. It is therefore well to show that without employing any other principles of inference than those already described, we may infer $A = AC$ from $A = ABC$, though we cannot infer the latter more full and accurate result from the former. We may show this most simply as follows:—

By the first law of thought it is evident that

$$AA = AA;$$

and if we have given the proposition $A = ABC$, we may substitute for both the A's in the second side of the above, obtaining

$$AA = ABC \,.\, ABC.$$

But from the property of logical symbols expressed in the

Law of Simplicity (p. 39) some of the repeated letters may be made to coalesce, and we have

$$A = ABC \,.\, C.$$

Substituting again for ABC its equivalent A, we obtain

$$A = AC,$$

the desired result.

By a similar process of reasoning it may be shown that we can always drop out any term appearing in one member of a proposition, provided that we substitute for it the whole of the other member. This process was described in my first logical Essay[h], as *Intrinsic Elimination*, but it might perhaps be better entitled the *Ellipsis of Terms*. It enables us to get rid of needless terms by strict substitutive reasoning.

Inference of a Simple from Two Partial Identities.

Two terms may be connected together by two partial identities in yet another manner, and a case of inference then arises which is of the highest importance. In the two premises

$$A = AB \qquad (1)$$
$$B = AB, \qquad (2)$$

the second member of each is the same; so that we can by obvious substitution obtain

$$A = B.$$

Thus in plain geometry we readily prove that 'Every equilateral triangle is also an equiangular triangle,' and we can with equal ease prove that 'Every equiangular triangle is an equilateral triangle.' Thence by substitution, as explained above, we pass to the simple identity—

Equilateral triangle = equiangular triangle.

We thus prove that one class of triangles is entirely identical with another class; that is to say, they differ only in our way of naming and regarding them.

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[h] 'Pure Logic,' p. 19.

The great importance of this process of inference arises from the fact that the conclusion is more simple and general than either of the premises, and contains as much information as both of them put together. It is on this account constantly employed in inductive investigation, as will afterwards be more fully explained, and it is the natural mode by which we arrive at a conviction of the truth of simple identities as existing between classes of numerous objects.

Inference of a Limited from Two Partial Identities.

We have just considered arguments which are of the type treated by Aristotle in the first figure of the syllogism. But there are two other types of argument which employ a pair of partial identities. If our premises are, as shown in these symbols,

$$B = AB \qquad (1)$$
$$B = CB, \qquad (2)$$

we may substitute for B either by (1) in (2) or by (2) in (1), and by both modes we obtain the conclusion

$$AB = CB, \qquad (3)$$

a proposition of the kind which we have called a limited identity (p. 51). Thus, for example,

Potassium = potassium metal (1)
Potassium = potassium floating on water ; (2)

hence

Potassium metal = potassium floating on water. (3)

Now this is really a syllogism of the mood Darapti in the third figure, except that we obtain a conclusion of a much more exact character than the old syllogism gives. From the premises 'Potassium is a metal' and 'Potassium floats on water,' Aristotle would have inferred that 'Some metals float on water.' But if inquiry were made what the *some* metals are, the answer would certainly be 'Metal which is potassium.' Hence Aristotle's conclusion simply leaves out some of the information afforded in the premises; it even leaves us open to interpret the *some metals* in a wider sense than we are warranted in doing. From these distinct defects of the syllogism the process of substitution is free, and it only incurs the possible objection of being tediously minute and accurate.

Miscellaneous Forms of Deductive Inference.

The more simple and common forms of deductive reasoning having been exhibited and demonstrated on the principle of substitution, there remain many, in fact an indefinite number, which may be explained with nearly equal ease. Such as involve the use of disjunctive propositions will be deferred to a later chapter, and several of the syllogistic moods which include negative terms will be more conveniently treated after we have introduced the symbolic use of the second and third laws of thought.

We sometimes meet with a chain of propositions which allow of repeated substitution and form an argument called in the old logic a Sorites. Take, for instance, the premises

Iron is a metal (1)
Metals are good conductors of electricity (2)
Good conductors of electricity are useful for telegraphic purposes. (3)

It obviously follows that

Iron is useful for telegraphic purposes. (4)

Now if we take our letters thus—

A = Iron, B = metal, C = good conductor of electricity, D = useful for telegraphic purposes,

the premises will assume the form—

$$A = AB \qquad (1)$$
$$B = BC \qquad (2)$$
$$C = CD \qquad (3)$$

For B in (1) we can substitute its equivalent in (2), and for C in (2) we can substitute its equivalent in (3). We shall obtain as an intermediate result,

$$A = ABC,$$

and from this the complete conclusion

$$A = ABCD. \qquad (4)$$

The full interpretation is that *Iron is iron, metal, good conductor of electricity, useful for telegraphic purposes,* which is abridged in common language by the ellipsis of the circumstances which are not of immediate importance.

Instead of all the propositions being of one type, as in the last example, we may have a series of premises of various character; for instance

Common salt is sodium chloride (1)
Sodium chloride crystallizes in a cubical form (2)
What crystallizes in a cubical form does not possess the power of double refraction; (3)

it will follow that

Common salt does not possess the power of double refraction. (4)

Taking our letter-terms thus—

A = Common salt,
B = Sodium chloride,
C = Crystallizing in a cubical form,
D = Possessing the power of double refraction,

we may state the premises in the form

$$A = B, \qquad (1)$$

$$B = BC, \qquad (2)$$

$$C = Cd. \qquad (3)$$

Substituting by (2) in (1) and by (3) in (2) we obtain

$$A = BCd, \qquad (4)$$

which is a more precise version of the common conclusion.

We often meet with a series of propositions describing the qualities or circumstances of one same thing, and we may if we like combine them all into one proposition by the process of substitution. This case is, in fact, that which Archbishop Thomson has called 'immediate inference by the sum of several predicates,' and his example will serve my purpose well[i]. He describes copper as 'A metal, of a red colour, and disagreeable smell and taste, all the preparations of which are poisonous, which is highly malleable, ductile, and tenacious, with a specific gravity of about 8.83.' If we assign the letter A to copper, and the succeeding letters of the alphabet in succession to the series of predicates, we have nine distinct statements, of the form

$$A = AB\ (1) \quad A = AC\ (2) \quad A = AD\ (3) \ldots\ldots A = AK\ (9).$$

We can readily combine these propositions into one by substituting for A in the second side of (1) its expression in (2). We thus get

$$A = ABC,$$

and by repeating the process over and over again we obtain the single proposition

$$A = ABCDEFGHIJK.$$

But Dr. Thomson is mistaken in supposing that we can obtain in this manner a definition of copper. Strictly speaking, the above proposition is only a description of copper, and all the ordinary descriptions of substances in scientific works may be summed up in this form. Thus we may assert of the organic substances called Paraffins that they are all saturated hydrocarbons, incapable of uniting with other substances, produced by heating the alcoholic iodides with zinc, and so on. It may be shown that no amount of ordinary description can be equivalent to definition.

Fallacies.

I have hitherto been engaged in showing that all the forms of reasoning of the old syllogistic logic, and an indefinite number of other forms in addition, may be readily and clearly explained on the single principle of substitution. It is now desirable to show that the same principle would prevent us falling into fallacies. So long as we exactly observe the one rule of substitution of equivalents it will be impossible to commit a *paralogism*, or to break any one of the elaborate rules of the ancient system. One rule is thus proved to be as powerful as the six, eight, or more rules by which the correctness of syllogistic reasoning was guarded.

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[i] 'An Outline of the Laws of Thought,' Fifth Ed. p. 161.

It was a fundamental rule, for instance, that two negative premises could give no conclusion. If we take the propositions—

Granite is not a sedimentary rock, (1)
Basalt is not a sedimentary rock, (2)

we ought not to be able to draw any inference concerning the relation of granite and basalt. Taking our letter-terms thus

A = granite
B = sedimentary rock
C = basalt,

the premises may be expressed in the form

$$A \sim B \qquad (1)$$
$$C \sim B. \qquad (2)$$

We have in this form two statements of difference; but the principle of inference can only work with a statement of agreement or identity (p. 62). Thus our rule gives us no power whatever of drawing any inference.

It is to be remembered, indeed, that we claim the power of always turning a negative proposition into an affirmative one; and it might seem that the old rule of negative premises would be thus circumvented. Let us try. The premises (1) and (2) when affirmatively stated (see p. 54), will take the form

$$A = Ab \qquad (1)$$
$$C = Cb. \qquad (2)$$

The reader will find it impossible by the rule of substitution to discover a relation between A and C. Three terms occur in these premises, namely A, b, and C; but they are so combined that no term occurring in one has its exact equivalent stated in the other. No substitution can therefore be made, and the principle holds true. Fallacy is impossible.

It would be a mistake to suppose that the mere occurrence of negative terms in both premises render them incapable of yielding a conclusion. The old rules of logic informed us that from two negative premises no conclusion could be drawn, but it is a fact that the rule in this bare form does not hold universally true; and I am not aware that any precise explanation has been given of the conditions under which it is or is not imperative. Consider the following example—

Whatever is not metallic is not capable of powerful magnetic influence, (1)
Carbon is not metallic, (2)
Therefore, carbon is not capable of powerful magnetic influence. (3)

Here we have two distinctly negative premises (1) and (2), and yet they yield a perfectly valid negative conclusion (3). The syllogistic rule is actually falsified in its bare and general statement. In this and many other cases we can convert the propositions into affirmative ones which yield a conclusion. To show this let

A = carbon, B = metallic,
C = capable of powerful magnetic influence.

The premises readily take the form

$$b = bc \qquad (1)$$

$$A = Ab, \qquad (2)$$

and substitution for b in (2) by means of (1), gives the conclusion

$$A = Abc \qquad (3)$$

Our principle of inference then includes the rule of negative premises whenever it is true, and discriminates correctly between the cases where it does and does not apply.

The paralogism, anciently called *Undistributed Middle*, is also easily exhibited and infallibly avoided by our system. Let the premises be

Hydrogen is an element, (1)
All metals are elements. (2)

According to the syllogistic rules the middle term *element* is here undistributed, and no conclusion can be obtained; we cannot tell then whether hydrogen is or is not a metal. Represent the terms as follows—

A = hydrogen
B = element
C = metal.

The premises then become

$$A = AB \quad (1)$$
$$C = CB. \quad (2)$$

The reader will here, as in a former page (p. 75), find it impossible to make any substitution. The only term which occurs in both premises is B, but it is combined with different letters. For CB we cannot substitute the equivalent of AB. We have no right to decompose combinations; and if we adhere rigidly to the rule given, that if two terms are stated to be equivalent we may substitute one for the other, we cannot commit the fallacy. It is apparent that the form of premises given above is the same as that which we obtained by translating two negative premises into the affirmative form.

The old fallacy, technically called the Illicit Process of the Major Term, is more easy to commit and more difficult to detect than any other breach of the syllogistic rules. In our system it could hardly occur. From the premises

All planets are subject to gravity, (1)
Fixed stars are not planets, (2)

we might inadvertently but fallaciously infer that, 'Fixed stars are not subject to gravity.' To reduce the premises to symbolic form, let

A = planet
B = fixed star
C = subject to gravity ;

then we have the propositions

$$A = AC \qquad (1)$$
$$B = Ba. \qquad (2)$$

The reader will try in vain to produce from these premises by legitimate substitution any relation between B and C ; he could not then commit the fallacy of asserting that B is not C.

There remain two other kinds of paralogism, commonly known as the fallacy of Four Terms and the Illicit Process of the Minor term. They are so evidently impossible while we obey the rule of the substitution of equivalents, that it is not necessary to give any illustrations. When there are four distinct terms in two propositions there could be no opening for a substitution. As to the Illicit Process of the Minor it consists in a flagrant substitution for a term of another wider term which is not known to be equivalent to it, and which is therefore forbidden by our rule to be substituted for it.

CHAPTER V.

DISJUNCTIVE PROPOSITIONS.

In the previous chapter I have exhibited various forms of deductive reasoning by the process of substitution, so far as they can be treated without the use of disjunctive propositions; but we cannot long defer the consideration of this more complex class of identities. General terms arise, as we have seen (p. 29), from classifying or mentally uniting together all objects which agree in certain qualities, the value of this union consisting in the fact that the power of knowledge is multiplied thereby. In forming such classes or general notions, we overlook or abstract the points of difference which exist between the objects joined together, and fix our attention only on the points of agreement. But every process of thought may be said to have its inverse process, which consists in undoing the effects of the direct process. Just as division undoes multiplication, and evolution undoes involution, so we must have a process which undoes abstraction, or the operation of forming general notions. This inverse process will consist in distinguishing the separate objects or minor classes which are the constituent parts of any wider class. When we mentally unite together certain objects visible in the sky and call them planets, we shall afterwards need to distinguish the contents of this general notion, which we do in the disjunctive proposition—

> A planet is either Mercury or Venus or the Earth or
> ..... or Neptune.

Having formed the very wide class 'vertebrate animal,' we may specify its subordinate classes thus:—'A vertebrate animal is either a mammalian, bird, reptile, or fish.' Nor is there any limit to the number of possible alternatives. 'An exogenous plant is either a ranunculus, a poppy, a crucifer, a rose, or it belongs to some one of the other seventy natural orders of exogens at present recognised by botanists.' A cathedral church in England must be either that of London, Canterbury, Winchester, Salisbury, Manchester, or of one of about twenty-four cities possessing such churches. And if we were to attempt to specify the meaning of the term 'star,' we should require to enumerate as alternatives, not only the many thousands of stars recorded in catalogues, but the many millions yet unnamed.

Whenever we thus distinguish the parts of a general notion we employ a disjunctive proposition, in at least one side of which are several alternatives joined by the so called disjunctive conjunction *or*, a contracted form of *other*. There must be some relation between the parts thus connected in one proposition; we may call it the *disjunctive* or *alternative* relation, and we must carefully inquire into its nature and results. This relation is that of doubt and ignorance, giving rise to choice or uncertainty. Whenever we classify and abstract we must open the way to such uncertainty. By fixing our attention on certain attributes to the exclusion of others we necessarily leave it doubtful what those other attributes are. The term 'molar tooth' bears upon the face of it that it is a part of the wider term 'tooth.' But if we meet with the simple term 'tooth' there is nothing to indicate whether it is an incisor, a canine, or a molar tooth. This doubt, however, may be resolved by other information, and we have to consider what are the appropriate logical processes for treating disjunctive propositions in connection with other propositions disjunctive or otherwise.

Expression of the Alternative Relation.

In order to represent disjunctive propositions with convenience we require a sign of the alternative or disjunctive relation, equivalent to one meaning at least of the little conjunction *or* so frequently used in common language. I propose to use for this purpose the symbol + . In my first logical Essay I followed the example of Dr. Boole and adopted the common sign + ; but this sign should not be employed unless there exists exact analogy between mathematical addition and logical alternation. We shall find that the analogy is of a very partial character, and that there is such profound difference between a logical and a mathematical term as should prevent our uniting them by the same symbol. Accordingly I have chosen a sign +, which seems aptly to suggest whatever degree of analogy may exist without implying more. The exact meaning of the symbol we will now proceed to investigate and determine.

Nature of the Alternative Relation.

Before treating disjunctive propositions it is indispensable to decide whether the alternatives shall be considered exclusive or unexclusive. By *exclusive alternatives* we mean those which cannot contain the same things. Thus

Matter is solid, or liquid, or gaseous ;

but the same portion of matter cannot be at once solid and liquid, properly speaking ; still less can we suppose it to be solid and gaseous, or solid, liquid and gaseous all at the same time. Many examples on the other hand can readily be suggested in which two or more alternatives may hold true of the same object. Thus

Luminous bodies are self-luminous or luminous by reflection.

It is undoubtedly possible by the laws of optics, that the same surface may at one and the same moment give off light of its own and reflect the light from other bodies. We speak familiarly of *deaf or dumb* persons, knowing that the majority of those who are deaf from birth are also dumb.

There can be no doubt that in a great many cases, perhaps the greater number of cases, alternatives are exclusive as a matter of fact. Any one number is incompatible with any other; one point of time or place is exclusive of all others. Roger Bacon died either in 1284 or 1292; it is certain that he could not die in both years. Henry Fielding was born either in Dublin or Somersetshire; he could not be born in both places. There is so much more precision and clearness in the use of exclusive alternatives that we ought doubtless to select them when possible. Old works on logic accordingly contained a rule directing that the *Membra dividentia*, the parts of a division or the constituent species of a genus should be exclusive of each other.

It is no doubt owing to the great prevalence and convenience of exclusive divisions that the majority of logicians have held it necessary to make every alternative in a disjunctive proposition exclusive of every other one. Aquinas considered that when this was not the case the proposition was actually *false*, and Kant adopted the same opinion[a]. A multitude of statements to the same effect might readily be quoted, and if the question were to be determined by the weight of historical evidence, it would certainly go against my view. Among recent logicians Sir W. Hamilton, as well as Dr. Boole, took the exclusive side. But there are authorities to the opposite effect. Whately, Mansel, and J. S. Mill, have all pointed out that we may often treat alternatives as *Compossible*, or true at the same time. Whately gives as an example[b], 'Virtue tends to procure us either the esteem of mankind, or the favour of God,' and he adds, 'Here both members are true, and consequently from one being affirmed we are not authorized to deny the other. Of course we are left to conjecture in each case, from the context, whether it is meant to be implied that the members are or are not exclusive.' Mansel says[c], '*We may happen to know* that two alternatives cannot be true together, so that the affirmation of the second necessitates the denial of the first; but this, as Boethius observes, is a *material*, not a *formal* consequence.' Mr. J. S. Mill has also pointed out the absurdities which would arise from always interpreting alternatives as exclusive. 'If we assert,' he says[d], 'that a man who has acted in some particular way must be either a knave or a fool, we by no means assert, or intend to assert, that he cannot be both.' Again, 'to make an entirely unselfish use of despotic power, a man must be either a saint or a philosopher. . . . . . Does the disjunctive premise necessarily imply, or must it be construed as supposing, that the same person cannot be both a saint and a philosopher? Such a construction would be ridiculous.'

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[a] Mansel's 'Aldrich,' p. 103, and 'Prolegomena Logica,' p. 221.

I discuss this subject fully because it is really the point which separates my logical system from that of the late Dr. Boole. In his 'Laws of Thought' (p. 32) he expressly says, 'In strictness, the words "and," "or," interposed between the terms descriptive of two or more classes of objects, imply that those classes are quite distinct, so that no member of one is found in another.' This I altogether dispute. In the ordinary use of these conjunctions we do not necessarily join distinct terms only; and when terms so joined do prove to be logically distinct, it is by virtue of a *tacit premise*, something in the meaning of the names and our knowledge of them, which teaches us they are distinct. And when our knowledge of the meanings of the words joined is defective it will often be impossible to decide whether terms joined by conjunctions are exclusive or not.

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[b] 'Elements of Logic,' Book II. chap. iv. sect. 4.

[c] Aldrich, 'Artis Logicæ Rudimenta,' p. 104.

[d] 'Examination of Sir W. Hamilton's Philosophy,' pp. 452–454.

Take, for instance, the proposition 'A peer is either a duke, or a marquis, or an earl, or a viscount, or a baron.' If expressed in Professor Boole's symbols, it would be implied that a peer cannot be at once a duke and marquis, or marquis and earl. Yet many peers do possess two or more titles, and the Prince of Wales is Duke of Cornwall, Earl of Dublin, and Baron Renfrew. If it were enacted by parliament that no peer should have more than one title, this would be the tacit premise which Professor Boole assumes to exist. Nor is the restriction true of more common terms.

In the sentence 'Repentance is not a single act, but a habit or virtue,' it cannot be implied that a virtue is not a habit; by Aristotle's definition it is.

Milton has the expression in one of his sonnets, 'Unstain'd by gold or fee,' where it is obvious that if the fee is not always gold, the gold is meant to be a fee or bribe.

Tennyson has the expression 'wreath or anadem.' Most readers would be quite uncertain whether a wreath may be an anadem, or an anadem a wreath, or whether they are quite distinct or quite the same.

From Darwin's 'Origin,' I take the expression, 'When we see any *part or organ* developed in a remarkable *degree or manner.*' In this, *or* is used twice, and neither time disjunctively. For if *part* and *organ* are not synonymous, at any rate an organ is a part. And it is obvious that a part may be developed at the same time both in an extraordinary degree and manner, although such cases may be comparatively very rare.

From a careful examination of ordinary writings, it will thus be found that the meanings of terms joined by 'and' 'or' vary from absolute identity up to absolute contrariety. There is no logical condition of distinctness at all, and when we do choose exclusive expressions, it is because our subject demands it. The matter, not the form of an expression, points out whether terms are exclusive[e]. The question, as we shall afterwards see, is one of the greatest theoretical importance, because it furnishes the true distinction between the sciences of Logic and Mathematics. It is the very foundation of number that every unit shall be distinct from every other unit; but Dr. Boole imported the conditions of number into the science of Logic, and produced a system which, though wonderful in its results, was not a system of logic at all.

Laws of the Disjunctive Relation.

In considering the combination or synthesis of terms (p. 39), we found that certain laws, those of Simplicity and Commutativeness, must be observed. In uniting terms by the disjunctive symbol we shall find that the same or closely similar laws hold true. The alternatives of either member of a disjunctive proposition are certainly commutative. Just as we cannot properly distinguish between *rich and rare gems* and *rare and rich gems*, so we must consider as identical the expression *rich or rare gems*, and *rare or rich gems*. In our symbolic language we may say generally

$$A \cdot\!|\!\cdot B = B \cdot\!|\!\cdot A.$$

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[e] 'Pure Logic,' pp. 76, 77.

The order of statement, in short, has no effect upon the meaning of an aggregate of alternatives, so that the Law of Commutativeness holds true of the disjunctive symbol.

As we have admitted the possibility of joining as alternatives terms which are not really different, the question arises, How shall we treat two or more alternatives when they are clearly shown to be the same? If we have it asserted that P is Q or R, and it is afterwards proved that Q is but another name for R, the result is that P is either R or R. How shall we interpret such a statement? What would be the meaning, for instance, of 'wreath or anadem' if, on referring to a dictionary, we found *anadem* described as a wreath? I take it to be self-evident that the meaning would then become simply 'wreath.' Accordingly we may affirm the general law

$$A \cdot\!|\!\cdot A = A.$$

Any number of identical alternatives may always be reduced to, and are logically equivalent to, any one of those alternatives. This is a law which distinguishes mathematical terms from logical terms, because it obviously does not apply to the former. I propose to call it the *Law of Unity*, because it must really be involved in any definition of a mathematical unit. This law is closely analogous to the Law of Simplicity, AA = A; and the nature of the connection is worthy of attention.

I am not aware that logicians have in any adequate way noticed the close relation between combined and disjunctive terms, namely that every disjunctive term is the negative of a corresponding combined term, and *vice versâ*. Consider the term

Malleable dense metal.

How shall we describe the class of things which are not malleable-dense-metals? Whatever is included under that term must have all the qualities of malleability, denseness, and metallic nature. Wherever any one or more of the qualities is wanting, the combined term will not apply. Hence the negative of the whole term is

Not-malleable or not-dense or not-metallic.

In the above the conjunction *or* must clearly be interpreted as unexclusive; for there may readily be objects which are both not-malleable, and not-dense, and perhaps not-metallic at the same time. If in fact we were required to use *or* in a strictly exclusive manner, it would be requisite to specify seven distinct alternatives in order to describe the negative of a combination of three single terms. The negatives of four or five terms would consist of fifteen or thirty-one alternatives. This consideration alone is sufficient to prove that the meaning of *or* cannot be always exclusive in common language.

Expressed symbolically, we may say that the negative of

A B C

is not-A or not-B or not-C;

that is, $a + b + c$.

Reciprocally the negative of

$$P + Q + R$$

is pqr.

Every disjunctive term, then, is the negative of a combined term, and *vice versâ*.

Apply this result to the combined term AAA, and its negative is

$$a + a + a.$$

Now since AAA is by the Law of Simplicity equivalent to A, so $a + a + a$ must be by the Law of Unity equivalent to a. Each law thus necessarily presupposes the other.

Symbolic expression of the Law of Duality.

We may now employ our symbol of alternation to express in a clear and formal manner the third Fundamental Law of Thought, which I have called the Law of Duality. Taking A to represent any class or object or quality, and B any other class, object or quality, we may always assert that A either agrees with B, or does not agree. Thus we may say

$$A = AB + Ab.$$

This is a formula which will henceforth be constantly employed, and it lies at the basis of reasoning.

The reader may perhaps wish to know why A is inserted in both alternatives of the second member of the identity, and why the law is not stated in the form

$$A = B + b.$$

But if he will consider the contents of the last section (p. 87), he will see that the latter expression cannot be correct, otherwise no term would have any negative. For the negative of $B + b$ is bB, or a self-contradictory term; so that if A were identical with $B + b$, its negative a would be non-existent. This result would generally be an absurd one, and I see much reason to think that in a strictly logical point of view it would always be absurd. In all probability we ought to assume as a fundamental logical axiom that *every term has its negative in thought.* We cannot think at all without separating what we think about from other different things, and these things necessarily form the negative notion[f]. If so, it follows that any term of the form $B + b$ is just as self-contradictory as one of the form Bb.

It will be convenient to recapitulate in this place the three great Laws of Thought in their symbolic form, thus

Law of Identity	$A = A.$
Law of Contradiction	$Aa = 0.$
Law of Duality	$A = AB + Ab.$

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[f] 'Pure Logic,' p. 65. See also the criticism of this point by De Morgan in the 'Athenæum,' No. 1892, 30th January, 1864; p. 155.

Various Forms of the Disjunctive Proposition.

Disjunctive propositions may occur in a great variety of forms, of which the old logicians took very insufficient notice. There may be any number of alternatives each of which may be a combination of any number of simple terms. A proposition, again, may be disjunctive in one or both members. The proposition

> Solids or liquids or gases are electrics or conductors of electricity

is an example of the doubly disjunctive form. The meaning of any such proposition is that whatever falls under any one or more alternatives on one side must fall under one or more alternatives on the other side. From what has been said before, it is apparent that the proposition

$$A + B = C + D$$

will correspond to

$$ab = cd,$$

each member of the latter being the negative of a member of the former proposition.

As an instance of a complex disjunctive proposition I may give Senior's definition of wealth, namely 'Wealth is what is transferable, limited in supply, and either productive of pleasure or preventive of pain[g].'

Let A = wealth
B = transferable
C = limited in supply
D = productive of pleasure
E = preventive of pain.

The definition takes the form

$$A = BC(D + E);$$

but if we develop the alternatives by a method to be afterwards more fully considered, it becomes

$$A = BCDE + BCDe + BCdE.$$

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[g] Boole's 'Laws of Thought,' p. 106. Jevons' 'Pure Logic,' p. 60.

An example of a still more complex proposition may be found in De Morgan's writings[h], and is as follows:—'He must have been rich, and if not absolutely mad was weakness itself, subjected either to bad advice or to most unfavourable circumstances.'

If we assign the letters of the alphabet in succession, thus,

A = he
B = rich
C = absolutely mad
D = weakness itself
E = subjected to bad advice
F = subjected to most unfavourable circumstances,

the proposition will take the form

$$A = AB\{C + D\,(E + F)\},$$

and if we develop the alternatives, expressing some of the different cases which may happen, we obtain

$$A = ABC + ABcDEF + ABcDEf + ABcDeF.$$

Inference by Disjunctive Propositions.

Before we can make a free use of disjunctive propositions in the processes of inference we must consider how disjunctive terms can be combined together or with simple terms. In the first place, to combine a simple term with a disjunctive one, we must combine it with every alternative of the disjunctive term. A vegetable, for instance, is either a herb, a shrub, or a tree. Hence an exogenous vegetable is either an exogenous herb, or an exogenous shrub, or an exogenous tree. Symbolically stated this process of combination is as follows—

$$A(B + C) = AB + AC.$$

Secondly, to combine two disjunctive terms with each other, combine each alternative of one separately with each alternative of the other. Since flowering plants are either exogens or endogens, and are at the same time either herbs, shrubs or trees, it follows that there are altogether six alternatives—namely, exogenous herbs, exogenous shrubs, exogenous trees, endogenous herbs, endogenous shrubs, endogenous trees. This process of combination is shown in the general form

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[h] 'On the Syllogism,' No. iii. p. 12. Camb. Phil. Trans., vol. x. part i.

$$(A + B)(C + D) = AC + AD + BC + BD.$$

It is hardly necessary to point out that, however numerous the terms combined, or the alternatives in those terms, we may effect the combination provided each alternative is combined with each alternative of the other terms, as in the algebraic process of multiplication.

Some processes of deduction may at once be exhibited. We may always, for instance, unite the same qualifying term to each side of an identity even though one or both members of the identity be disjunctive. Thus let

$$A = B + C.$$

Now it is self-evident that

$$AD = AD,$$

and in one side of this identity we may for A substitute its equivalent B + C obtaining

$$AD = BD + CD.$$

Since 'a gaseous element is either hydrogen, or oxygen, or nitrogen, or chlorine, or fluorine,' it follows that 'a free gaseous element is either free hydrogen, or free oxygen, or free nitrogen, or free chlorine, or free fluorine.'

This process of combination will lead to most useful inferences when the qualifying adjective combined with both sides of the proposition is a negative of one or more alternatives. Since chlorine is a coloured gas, we may infer that 'a colourless gaseous element is either (colourless) hydrogen, oxygen, nitrogen, or fluorine.' The alternative chlorine disappears because colourless chlorine does not exist. Again, since 'a tooth is either an incisor, canine, bicuspid, or molar,' it follows that 'a not-incisor tooth is either canine, bicuspid, or molar.' The general rule is that from the denial of any of the alternatives the affirmation of the remainder can be inferred. Now this result clearly follows from our process of substitution; for if we have the proposition

$$A = B + C + D,$$

and insert this expression for A on one side of the self-evident identity

$$Ab = Ab,$$

we obtain $Ab = ABb + AbC + AbD$;

and, as the first of the three alternatives is self-contradictory, we strike it out according to the law of contradiction: there remains

$$Ab = AbC + AbD.$$

Thus our system fully includes and explains that mood of the Disjunctive Syllogism technically called the *modus tollendo ponens.*

But the reader must carefully observe that the Disjunctive Syllogism of the mood *ponendo tollens,* which affirms one alternative, and thence infers the denial of the rest, cannot be held true in this system. If I say, indeed, that

Water is either salt or fresh water,

it seems evident that 'water which is salt is not fresh.' But this inference really proceeds from our knowledge that water cannot be at once salt and fresh. This inconsistency of the alternatives, as I have fully shown, will not always hold. Thus, if I say

Gems are either rare stones or beautiful stones, (1)

it will obviously not follow that

A rare gem is not a beautiful stone, (2)

nor that

A beautiful gem is not a rare stone. (3)

Our symbolic method gives only true conclusions; for if we take

A = gem
B = rare stone
C = beautiful stone,

the proposition (1) is of the form

$$A = B + C$$

hence $AB = B + BC$

and $AC = BC + C$;

but these inferences are not equivalent to the false ones (2) and (3).

We can readily represent such disjunctive reasoning, when it is valid, by expressing the inconsistency of the alternatives explicitly. Thus if we resort to our instance of

Water is either salt or fresh,

and take A = Water
B = salt
C = fresh,

then the premise is apparently of the form

$$A = AB + AC;$$

but in reality there are the unexpressed conditions that 'what is salt is not fresh,' and 'what is fresh is not salt;' or, in letter-terms,

$$B = Bc$$
$$C = bC.$$

Now, if we substitute these descriptions in the original proposition, we obtain

$$A = ABc + AbC;$$

uniting B to each side we infer

$$AB = ABc + ABbC$$

or $AB = ABc$;

that is,

Water which is salt is water salt and not fresh.

I should weary the reader if I attempted to illustrate the multitude of forms which disjunctive reasoning may take; and as in the next chapter we shall be constantly treating the subject, I must here restrict myself to a single instance. A very common process of reasoning consists in the determination of the name of a thing by the successive exclusion of alternatives, a process called by the old name *abscissio infiniti*. Take the case:—

Red-coloured metal is either copper or gold (1)
Copper is dissolved by nitric acid (2)
This specimen is red-coloured metal (3)
This specimen is not dissolved by nitric acid (4)
Therefore this specimen consists of gold. (5)

Assigning our letter-symbols thus —

A = this specimen
B = red-coloured metal
C = copper
D = gold
E = dissolved by nitric acid,

the premises may be stated in the form

$$B = BCd + BcD \quad (1)$$
$$C = CE \quad (2)$$
$$A = AB \quad (3)$$
$$A = Ae. \quad (4)$$

Substituting for C in (1) by means of (2) we get

$$B = BCdE + BcD.$$

From (3) and (4) we may infer likewise

$$A = ABe,$$

and if in this we substitute for B its equivalent just stated, it follows that

$$A = ABCdEe + ABcDe.$$

The first of the alternatives being contradictory, the result is
$$A = ABcDe$$
which contains a full description of 'this specimen,' as furnished in the premises, but by ellipsis indicates that it is gold. It will be observed that in the symbolic expression (1) I have explicitly stated what is certainly implied, that copper is not gold, and gold not copper, without which condition the inference would not hold good.

CHAPTER VI.

THE INDIRECT METHOD OF INFERENCE.

THE forms of deductive reasoning as yet considered, are mostly cases of Direct Deduction as distinguished from those which we are now about to treat. The method of Indirect Deduction may be described as that which points out what a thing is, by showing that it cannot be anything else. We can define a certain space upon a map, either by colouring that space, or by colouring all except the space; the first mode is positive, the second negative. The difference, it will be readily seen, is exactly analogous to that between the direct and indirect proof in geometry. Euclid often shows that two lines are equal, by showing that they cannot be unequal, and the proof rests upon the known number of alternatives, greater, equal or less, which are alone conceivable. In other cases, as for instance in the seventh proposition of the first book, he shows that two lines must meet in a particular point, by showing that they cannot meet elsewhere.

In logic we can always define with certainty the utmost number of alternatives which are conceivable. The Law of Duality (pp. 6, 88) enables us always to assert that any quality or circumstance whatsoever is either present or absent in anything. Whatever may be the meaning and nature of the terms A and B it is certainly true that

$$A = AB + Ab$$
$$B = AB + aB.$$

These are universal though tacit premises which may be employed in the solution of every problem, and which are such invariable and necessary conditions of all thought, that they need not be specially laid down. The Law of Contradiction is a further condition of all thought and of all logical symbols; it enables, and in fact obliges, us to reject from further consideration all terms which imply the presence and absence of the same quality. Now, whenever we bring both these Laws of Thought into explicit action by the method of substitution, we employ the Indirect Method of Inference. It will be found that we can treat not only those arguments already exhibited according to the direct method, but we can also include an infinite multitude of other arguments which are incapable of solution by any other means.

Some philosophers, especially those of France, have held that the Indirect Method of Proof has a certain inferiority to a direct method, which should prevent our using it except when obliged. But there are an unlimited number of truths which we can prove only indirectly. We can prove that a number is a prime only by the purely indirect method of showing that it is not any of the numbers which have divisors, and the remarkable process known as Eratosthenes' Sieve is the only mode by which we can select the prime numbers[a]. It bears a strong analogy to the indirect method here to be described. We can also prove that the side and diameter of a square are incommensurable, but only in the negative or indirect manner, by showing that the contrary supposition constantly and inevitably leads to contradiction[b]. Many other demonstrations in various branches of the mathematical sciences rest upon a like method. Now if there is only one important truth which must be, and can only be proved indirectly, we may say that the process is a necessary and sufficient one, and the question of its comparative excellence or usefulness is not worth discussion. As a matter of fact I believe that nearly half our logical conclusions rest upon its employment.

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[a] See Horsley, 'Philosophical Transactions,' 1772; vol. lxii. p. 327. Montucla, 'Histoire des Mathematiques,' vol. i. p. 239. 'Penny Cyclopædia,' article *Eratosthenes*.

[b] Euclid, Book x. Prop. 117.

Simple Illustrations.

In tracing out the powers and results of this method, we will begin with the simplest possible instance. Let us take a proposition of the very common form, A = AB, say,

A Metal is an Element,

and let us investigate its full meaning. Any person who has had the least logical training, is aware that we can draw from the above proposition an apparently different one, namely,

A Not-element is a Not-metal.

While some logicians, as for instance De Morgan,[c] have considered the relation of these two propositions to be purely self-evident, and neither needing nor allowing analysis, a great many more persons, as I have observed while teaching logic, are at first unable to perceive the close connection between them. I believe that a true and complete system of logic will furnish a clear analysis of this process which has been called *Contrapositive Conversion*; the full process is as follows:—

Firstly, by the Law of Duality we know that

Not-element is either Metal or Not-metal.

Now if it be metal, we know that it is by the premise *an element*; we should thus be supposing that the very same thing is an element and a not-element, which is in opposition to the Law of Contradiction. According to the only other alternative, then, the not-element must be a not-metal.

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[c] 'Philosophical Magazine,' December 1852, Fourth Series, vol. iv. p. 435. 'On Indirect Demonstration.'

To represent this process of inference symbolically we take the premise in the form

$$A = AB. \qquad (1)$$

We observe that by the Law of Duality the term not-B is thus described

$$b = Ab + ab. \qquad (2)$$

For A in this proposition we substitute its description as given in (1), obtaining

$$b = ABb + ab.$$

But according to the Law of Contradiction the term AB*b* must be excluded from thought or

$$ABb = 0.$$

Hence it results that *b* is either nothing at all, or it is *ab*; and the conclusion is

$$b = ab.$$

As it will often be necessary to refer to a conclusion of this kind I shall call it, as is usual, the *Contrapositive Proposition* of the original. The reader need hardly be cautioned to observe that from all A's are B's it does not follow that all not-A's are not-B's. For by the Law of Duality we have

$$a = aB + ab,$$

and it will not be found possible to make any substitution in this by our original premise A = AB. It still remains doubtful, therefore, whether not-metal is element or not-element.

The proof of the Contrapositive Proposition given above is exactly the same as that which Euclid applies in the case of geometrical notions. De Morgan describes Euclid's process as follows[d]:—'From every not-B is not-A he produces every A is B, thus—If it be possible, let this A be not-B, but every not-B is not-A, therefore this A is not-A, which is absurd: whence every A is B.' Now De Morgan thinks that this proof is entirely needless, because common logic gives the inference without the use of any geometrical reasoning. I conceive however that logic gives the inference only by an indirect process. De Morgan claims 'to see identity in every A is B and every not-B is not-A, by a process of thought prior to syllogism.' But whether prior to syllogism or not, I claim that it is not prior to the laws of thought and the process of substitutive inference by which it may be undoubtedly demonstrated.

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[d] 'Philosophical Magazine,' Dec. 1852; p. 437.

Employment of the Contrapositive Proposition.

We can frequently employ the contrapositive form of a proposition by the method of substitution; and certain moods of the ancient syllogism, which we have hitherto passed over, may thus be satisfactorily comprehended in our system. Take for instance the following syllogism in the mood Camestres:—

> 'Whales are not true fish: for they do not respire water, whereas true fish do respire water.'

Let us take

A = whales,
B = true fish,
C = respiring water.

The premises are of the form

$$A = Ac, \qquad (1)$$

$$B = BC. \qquad (2)$$

Now, by the process of contraposition we obtain from (2)

$$c = bc,$$

and we can substitute this expression for c in (1), obtaining

$$A = Abc,$$

or 'Whales are not true fish, not respiring water.'

The mood Cesare does not really differ from Camestres except in the order of the premises, and it could be exhibited in an exactly similar manner.

The mood Baroko gave much trouble to the old logicians who could not *reduce* it to the first figure in the same manner as the other moods, and were obliged to invent, specially for it and for Bokardo, a method of Indirect Reduction closely analogous to the Indirect proof of Euclid. Now these moods require no exceptional processes in this system. Let us take as an instance of Baroko, the argument

All heated solids give continuous spectra, (1)
Some nebulæ do not give continuous spectra; (2)
Therefore some nebulæ are not heated solids. (3)

Treating the little word *some* as an indeterminate adjective of selection, to which we assign a symbol like any other adjective, let

A = some
B = nebulæ
C = giving continuous spectra
D = heated solid.

The premises then become

$$D = DC \tag{1}$$

$$AB = ABc \tag{2}$$

Now from (1) we obtain by the Indirect method the Contrapositive

$$c = cd,$$

and if we substitute this expression for c in (2) we have

$$AB = ABcd;$$

the full meaning of which is that 'some nebulæ do not give continuous spectra and are not solids.'

We might similarly apply the contrapositive in many other instances. Take the argument—'All fixed stars are self-luminous; but some of the heavenly bodies are not self-luminous, and are therefore not fixed stars.' Taking our terms

A = fixed stars
B = self-luminous

C = some

D = heavenly bodies,

we have the premises

$$A = AB, \qquad (1)$$

$$CD = bCD. \qquad (2)$$

Now from (1) we can draw the Contrapositive

$$b = ab,$$

and substituting this expression for b in (2) we obtain

$$CD = abCD,$$

which expresses the conclusion of the argument that 'some heavenly bodies are not fixed stars.'

Contrapositive of a Simple Identity.

The reader should carefully note that when we apply the process of Indirect Inference to a simple identity of the form

$$A = B,$$

we may obtain further results. If we wish to know what is the term not-B, we have as before, by the Law of Duality,

$$b = Ab + ab,$$

and substituting for A we obtain

$$b = Bb + ab = ab.$$

But we may now also draw a second Contrapositive; for we have

$$a = aB + ab,$$

and substituting for B its equivalent A we have

$$a = aA + ab = ab.$$

Hence from the single identity $A = B$ we can draw the two propositions

$$a = ab$$

$$b = ab,$$

and observing that these propositions have a common term we can make a new substitution, getting

$$a = b.$$

This result is in strict accordance with the fundamental principles of inference, and it may be a question whether it is not a self-evident result, independent of the steps of deduction by which we have reached it. For where two classes are coincident like A and B, whatever is true of the one is true of the other; what is excluded from the one must be excluded from the other similarly. Now as *a* bears to A exactly the same relation that *b* bears to B, the identity of either pair follows from the identity of the other pair. In every identity, equality, or similarity, we may argue from the negative of the one side to the negative of the other. Thus at ordinary temperatures

Mercury = liquid-metal,

hence obviously

Not-mercury = not-liquid-metal;

or since

Sirius = brightest fixed star,

it follows that whatever star is not the brightest is not Sirius, and *vice versâ*. Every correct definition is of the form A = B, and may often require to be applied in the equivalent negative form.

Let us take as an illustration of the mode of using this result the argument following:—

Vowels are letters which can be sounded alone, (1)
The letter *w* cannot be sounded alone; (2)
Therefore the letter *w* is not a vowel. (3)

Here we have a definition (1), and a comparison of a thing with that definition (2), leading to exclusion of the thing from the class defined.

Taking the terms

A = vowel,
B = letter which can be sounded alone,
C = letter *w*,

the premises are plainly of the form

$$A = B, \qquad (1)$$

$$C = bC. \qquad (2)$$

Now by the Indirect method we obtain from (1) the Contrapositive

$$b = a,$$

and inserting in (2) the equivalent for b we have

$$C = aC, \qquad (3)$$

or 'the letter *w* is not a vowel.'

Miscellaneous Examples of the Method.

We can apply the Indirect Method of Inference however many may be the terms involved or the premises containing those terms. As the working of the method is best learnt from examples, I will take a case of two premises forming the syllogism Barbara: thus

Iron is a metal (1)
Metal is element. (2)

If we want to ascertain what inference is possible concerning the term *Iron*, we develop the term by the Law of Duality. Iron must be either metal or not-metal; iron which is metal must be either element or not-element; and similarly iron which is not-metal must be either element or not-element. There are then altogether four alternatives among which the description of iron must be contained; thus

Iron, metal, element, (α)
Iron, metal, not-element, (β)
Iron, not-metal, element, (γ)
Iron, not-metal, not-element. (δ)

Our first premise informs us that iron is a metal, and if we substitute this description in (γ) and (δ) we shall have self-contradictory combinations. Our second premise likewise informs us that metal is element, and applying this description to (β) we again have self-contradiction, so that there remains only (α) as a description of iron—our inference is

Iron = iron, metal, element.

To represent this process of reasoning in general symbols, let

A = iron
B = metal
C = element.

The premises of the problem take the form

$$A = AB \quad (1)$$
$$B = BC. \quad (2)$$

By the Law of Duality we have

$$A = AB + Ab \quad (3)$$
$$A = AC + Ac. \quad (4)$$

Now, if we insert for A in the second side of (3) its description in (4), we obtain what I shall call the *development* of A

$$A = ABC + ABc + AbC + Abc. \quad (5)$$

Wherever the letters A or B appear in the second side of (5) substitute their equivalents given in (1) and (2) and the results at full length are

$$A = ABC + ABCc + ABbC + ABbCc.$$

The last three alternatives break the Law of Contradiction, so that

$$A = ABC + 0 + 0 + 0$$
$$A = ABC.$$

This conclusion is, indeed, no more than we could obtain by the direct process of substitution; it is the characteristic of the Indirect process that it gives all possible logical conclusions, both those which we have previously obtained, and an almost infinite number of others of which the ancient logic took little or no account. From the same premises, for instance, we can obtain a description of the class *not-element* or c. By the Law of Duality we can develop c into four alternatives, thus—

$$c = ABc + Abc + aBc + abc.$$

Now if we substitute for A and B as before, we get

$$c = ABCc + ABbc + aBCc + abc,$$

and striking out the terms which break the Law of Contradiction there remains

$$c = abc,$$

or what is not element is also not iron and not metal. This Indirect Method of Inference thus furnishes a complete solution of the following problem—*Given any number of logical premises or conditions, required the description of any class of objects, or any term, as governed by those conditions.*

The steps of the process of inference may thus be concisely stated :—

1. By the Law of Duality develop the utmost number of alternatives which may exist in the description of the required class or term as regards the terms involved in the premises.
2. For each term in these alternatives substitute its description as given in the premises.
3. Strike out every alternative which is then found to break the Law of Contradiction.
4. The remaining terms may be equated to the term in question as the desired description or inference.

Abbreviation of the Process.

Before proceeding to illustrations of the use of this method, I must point out how much its practical employment can be simplified, and how much more easy it is than would appear from the description. When we want to effect at all a complete solution of a logical problem it is best to form, in the first place, a complete series of all the combinations of terms involved in it. If there be two terms A and B, the utmost variety of combinations in which they can appear are

AB
A*b*
*a*B
ab.

The term A appears in the first and second; B in the first and third; *a* in the third and fourth; and *b* in the second and fourth. Now if we have any premise, say

$$A = B,$$

we must ascertain which of these combinations would be rendered self-contradictory by substitution; the second and third would have to be struck out, and there would remain

AB
ab.

Hence we draw the following inferences

$$A = AB, \quad B = AB, \quad a = ab, \quad b = ab.$$

Exactly the same method must be followed where a question involves a greater number of terms. Thus by the Law of Duality the three terms A, B, C, give rise to eight conceivable combinations, namely

ABC (α)
AB*c* (β)
A*b*C (γ)
A*bc* (δ)
*a*BC (ε)
*a*B*c* (ζ)
*ab*C (η)
abc. (θ)

The development of the term A is formed by the first four of these; for B we must select (α), (β), (ε), (ζ); C consists of (α), (γ), (ε) (η); *b* of (γ), (δ), (η), (θ), and so on.

Now if we want to investigate completely the meaning of the premises

$$A = AB \qquad (1)$$
$$B = BC, \qquad (2)$$

we examine each of the eight combinations as regards each premise; (γ) and (δ) are contradicted by (1), and (β) and (ζ) by (2), so that there remain only

$$ABC \qquad (\alpha)$$
$$aBC \qquad (\epsilon)$$
$$abC \qquad (\eta)$$
$$abc. \qquad (\theta)$$

To describe any term under the conditions of the premises (1) and (2), we have only to draw out the proper combinations from this list; thus—A is represented only by ABC or

$$A = ABC,$$

similarly $c = abc.$

For B we have two alternatives thus stated,

$$B = ABC + aBC\,;$$

and for b we have

$$b = abC + abc.$$

When we have a problem involving four distinct terms we need to double the number of combinations, and as we add each new term the combinations become twice as numerous. Thus

A, B	produce	four	combinations
A, B, C,	„	eight	„
A, B, C, D	„	sixteen	„
A, B, C, D, E	„	thirty-two	„
A, B, C, D, E, F	„	sixty-four	„

and so on.

I propose to call any such series of combinations the *Logical Abecedarium.* It holds in logical science a position of importance which cannot be exaggerated. As we proceed from logical to mathematical considerations it will become apparent that there is a close connection between these combinations and the most fundamental theorems of mathematical science. For the convenience of the reader who may wish to employ the abecedarium in logical questions, I have had printed on the next page a complete series of the combinations up to those of six terms. At the very commencement in the first column is placed a single letter X which might seem to be superfluous. This letter serves to denote that it is always some higher class which is divided up. Thus the combination AB really means ABX, or that part of some larger class, say X, which has the qualities of A and B present. The letter X is omitted in the greater part of the table merely for the sake of brevity and clearness. In a later chapter on Combinations it will become apparent that the introduction of this unit class is requisite in order to complete the analogy with the Arithmetical Triangle there described.

The reader ought to bear in mind that though the abecedarium seems to give mere lists of combinations, these combinations are intended in every case to constitute the development of a term of a proposition. Thus the four combinations AB, A*b*, *a*B, *ab* really mean that any class X is described by the following proposition,

$$X = X(AB + Ab + aB + ab).$$

If we select the A's, we obtain the following proposition

$$AX = X(AB + Ab).$$

Thus whatever group of combinations we treat must be conceived as part of a higher class, *summum genus* or universe symbolised in the term X; but bearing this in mind, it is needless to complicate our formulæ by always introducing the letter. All inference consists in passing from propositions to propositions, and combinations *per se* have no meaning. They are consequently to be regarded in all cases as forming parts of propositions.

THE LOGICAL ABECEDARIUM.

I.	II.	III.	IV.	V.	VI.	VII.
X	A X	A B	A B C	A B C D	A B C D E	A B C D E F
	a X	A *b*	A B *c*	A B C *d*	A B C D *e*	A B C D E *f*
		a B	A *b* C	A B *c* D	A B C *d* E	A B C D *e* F
		a *b*	A *b* *c*	A B *c* *d*	A B C *d* *e*	A B C D *e* *f*
			a B C	A *b* C D	A B *c* D E	A B C *d* E F
			a B *c*	A *b* C *d*	A B *c* D *e*	A B C *d* E *f*
			a *b* C	A *b* *c* D	A B *c* *d* E	A B C *d* *e* F
			a *b* *c*	A *b* *c* *d*	A B *c* *d* *e*	A B C *d* *e* *f*
				a B C D	A *b* C D E	A B *c* D E F
				a B C *d*	A *b* C D *e*	A B *c* D E *f*
				a B *c* D	A *b* C *d* E	A B *c* D *e* F
				a B *c* *d*	A *b* C *d* *e*	A B *c* D *e* *f*
				a *b* C D	A *b* *c* D E	A B *c* *d* E F
				a *b* C *d*	A *b* *c* D *e*	A B *c* *d* E *f*
				a *b* *c* D	A *b* *c* *d* E	A B *c* *d* *e* F
				a *b* *c* *d*	A *b* *c* *d* *e*	A B *c* *d* *e* *f*
					a B C D E	A *b* C D E F
					a B C D *e*	A *b* C D E *f*
					a B C *d* E	A *b* C D *e* F
					a B C *d* *e*	A *b* C D *e* *f*
					a B *c* D E	A *b* C *d* E F
					a B *c* D *e*	A *b* C *d* E *f*
					a B *c* *d* E	A *b* C *d* *e* F
					a B *c* *d* *e*	A *b* C *d* *e* *f*
					a *b* C D E	A *b* *c* D E F
					a *b* C D *e*	A *b* *c* D E *f*
					a *b* C *d* E	A *b* *c* D *e* F
					a *b* C *d* *e*	A *b* *c* D *e* *f*
					a *b* *c* D E	A *b* *c* *d* E F
					a *b* *c* D *e*	A *b* *c* *d* E *f*
					a *b* *c* *d* E	A *b* *c* *d* *e* F
					a *b* *c* *d* *e*	A *b* *c* *d* *e* *f*
						a B C D E F
						a B C D E *f*
						a B C D *e* F
						a B C D *e* *f*
						a B C *d* E F
						a B C *d* E *f*
						a B C *d* *e* F
						a B C *d* *e* *f*
						a B *c* D E F
						a B *c* D E *f*
						a B *c* D *e* F
						a B *c* D *e* *f*
						a B *c* *d* E F
						a B *c* *d* E *f*
						a B *c* *d* *e* F
						a B *c* *d* *e* *f*
						a *b* C D E F
						a *b* C D E *f*
						a *b* C D *e* F
						a *b* C D *e* *f*
						a *b* C *d* E F
						a *b* C *d* E *f*
						a *b* C *d* *e* F
						a *b* C *d* *e* *f*
						a *b* *c* D E F
						a *b* *c* D E *f*
						a *b* *c* D *e* F
						a *b* *c* D *e* *f*
						a *b* *c* *d* E F
						a *b* *c* *d* E *f*
						a *b* *c* *d* *e* F
						a *b* *c* *d* *e* *f*

In a theoretical point of view we may conceive that the abecedarium is always extended indefinitely. Every new quality or circumstance which can belong to an object, subdivides each combination or class, so that the number of such combinations when unrestricted by logical conditions is represented by an indefinitely high power of two. The extremely rapid increase in the number of subdivisions obliges us to confine our attention to a few circumstances at a time.

When contemplating the properties of this abecedarium, I am often inclined to think that Pythagoras perceived the deep logical importance of duality; for while unity was the symbol of identity and harmony, he described the number two as the origin of contrasts, or the symbol of diversity, division and separation. The number four or the *Tetractys* was also regarded by him as one of the chief elements of existence, for it represented the generating virtue whence come all combinations.

In one of the golden verses ascribed to Pythagoras, he conjures his pupil to be virtuous[e]:

> 'By him who stampt *The Four* upon the Mind,
> *The Four*, the fount of Nature's endless stream.'

Now four and the higher powers of duality do represent in this logical system the variety of combinations which can be generated in the absence of logical restrictions. The followers of Pythagoras may have shrouded their master's doctrines in mysterious and superstitious notions, but in many points these doctrines seem to have some basis in logical philosophy.

The Logical Slate.

To a person who has once comprehended the extreme significance and utility of the Logical Abecedarium, the indirect process of inference becomes reduced to the repetition of a few uniform operations of classification, selection, and elimination of contradictories. Logical deduction even in the most complicated questions becomes a matter of mere routine, and the amount of labour required is the only impediment when once the meaning of the premises is rendered clear. But the amount of labour is often found to be considerable. The mere writing down of sixty-four combinations of six letters each is no small task, and, if we had a problem of five premises, each of the sixty-four combinations would have to be examined in connection with each premise. The requisite comparison is often of a very tedious character and considerable chance of errors thus arises.

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[e] Whewell, 'History of the Inductive Sciences,' vol. i. p. 222.

I have given much attention therefore to reducing both the manual and mental labour of the process, and I shall describe several devices which may be adapted for saving trouble and risk of mistake.

In the first place, as the same sets of combinations occur over and over again in different problems, we may avoid the labour of writing them out by having the sets of letters ready printed upon small sheets of writing paper. It has also been suggested by a correspondent that, if any one series of combinations were marked upon the margin of a sheet of paper, and a slit cut between each pair of combinations, it would be easy to fold down any particular combination, and thus strike it out of view. The combinations consistent with the premises would then remain in a broken series. This method answers sufficiently well for occasional use.

A more convenient mode, however, is to have the series of letters shown on p. 109, engraved upon a common school writing slate, of such a size, that the letters may occupy only about a third of the space on the left hand side of the slate. The conditions of the problem can then be written down on the unoccupied part of the slate, and the proper series of combinations being chosen, the contradictory combinations can be struck out with the pencil. I have used a slate of this kind, which I call a *Logical Slate*, for more than ten years, and it has saved me much trouble. It is hardly possible to apply this process to problems of more than six terms, owing to the large number of combinations which would require examination; thus seven terms would give 128 combinations, eight terms 256, nine terms 512, ten terms 1024, eleven terms 2048, twelve terms 4096, and so on in geometrical progression.

Abstraction of Indifferent Circumstances.

There is a simple but highly important process of inference which enables us to abstract, eliminate or disregard all circumstances indifferently present and absent. Thus if I were to state that 'a triangle is a figure of three sides, with or without equal angles,' the latter qualification would be superfluous, because by a law of thought I know that angles must be either equal or unequal. To add the qualification gives no new knowledge since the existence of the two alternatives will be understood in the absence of any information to the contrary. Accordingly, when two alternatives differ only as regards a single component term which is positive in one and negative in the other, we may always reduce them to one term by striking out their indifferent part. It is really a process of substitution which enables us to do this; for having any proposition of the form

$$A = ABC + ABc, \qquad (1)$$

we know by the Law of Duality that

$$B = BC + Bc. \qquad (2)$$

Hence

$$AB = ABC + ABc. \qquad (3)$$

And as the second member of this is identical with the second member of (1) we may substitute, obtaining

$$A = AB.$$

This process of reducing useless alternatives, may be applied again and again; for it is plain that

$$A = AB\,(CD + Cd + cD + cd)$$

communicates no more information than that A is B. This abstraction of indifferent terms is in fact the converse process to that of development described in p. 104; and it is one of the most important operations in the whole sphere of reasoning.

The reader should observe that in the proposition

$$AC = BC$$

we cannot abstract C and infer

$$A = B;$$

but from

$$AC + Ac = BC + Bc$$

we may abstract all reference to the term C.

Illustrations of the Indirect Method.

An infinite variety of arguments and logical problems might be introduced here to show the comprehensive character and powers of the Indirect Method. We can treat either a single premise or a series of premises.

Take in the first place a simple definition, such as 'a triangle is a three-sided rectilinear figure.' Let

A = triangle
B = three-sided
C = rectilinear figure,

then the definition is of the form

$$A = BC.$$

If we take the series of eight combinations of three letters (see p. 106) and strike out those which are inconsistent with the definition, we have the following result :—

ABC ~~*a*BC~~
~~AB*c*~~ *a*B*c*
~~A*b*C~~ *ab*C
~~A*bc*~~ *abc*.

For the description of the class C we have

$$C = ABC + abC,$$

that is, 'a rectilinear figure is either a triangle and three-sided, or not a triangle and not three-sided.'

For the class *b* we have

$$b = abC + abc.$$

To the second side of this we may apply the process of simplification by abstraction described in the last section; for by the Law of Duality

$$ab = abC + abc;$$

and as we have two propositions identical in the second side of each we may substitute, getting

$$b = ab,$$

or what is not three-sided is not a triangle (whether it be rectilinear or not).

Let us treat by this method the following argument :—

> 'Blende is not an elementary substance; elementary substances are those which are undecomposable; blende, therefore, is decomposable.'

Taking our letters thus—

A = blende,
B = elementary substance,
C = undecomposable,

the premises are of the form

$$A = Ab, \qquad (1)$$

$$B = C. \qquad (2)$$

No immediate substitution can be made; but if we take the contrapositive of (2), namely

$$b = c, \qquad (3)$$

we can substitute in (1) obtaining the conclusion

$$A = Ac.$$

But the same result may be obtained by taking the eight combinations of A, B, C, of the abecedarium; it will be found that only three combinations, namely

$$Abc$$
$$aBC$$
$$abc,$$

are consistent with the premises, whence it results that

$$A = Abc,$$

or by the process of Ellipsis before described (p. 69)

$$A = Ac.$$

As a somewhat more complex example I take the argument thus stated, one which could not be thrown into the syllogistic form.

> 'All metals except gold and silver are opaque; therefore what is not opaque is either gold or silver or is not-metal.'

There is more implied in this statement than is distinctly asserted, the full meaning being as follows:

All metals not gold or silver are opaque, (1)
Gold is not opaque but is a metal. (2)
Silver is not opaque but is a metal, (3)
Gold and silver are distinct substances. (4)

Taking our letters thus—

A = metal C = silver
B = gold D = opaque,

we may state the premises in the form

$$Abc = AbcD \quad (1)$$
$$B = ABd \quad (2)$$
$$C = ACd \quad (3)$$
$$B = Bc. \quad (4)$$

To obtain a complete solution of the question we take the sixteen combinations of A, B, C, D, and striking out those which are inconsistent with the premises, there remain only

$$\begin{array}{l} ABcd \\ AbCd \\ AbcD \\ abcD \\ abcd. \end{array}$$

The expression for not-opaque things consists of the three combinations containing d, thus

$$d = ABcd + AbCd + abcd,$$

or

$$d = Ad\,(Bc + bC) + abcd.$$

In ordinary language, what is not-opaque is either metal which is gold, and then not-silver, or silver and then not gold, or else it is not-metal and neither gold nor silver.

A good example for the illustration of the Indirect Method is to be found in De Morgan's Formal Logic (p. 123), the premises being substantially as follows:—

From A follows B, and from C follows D; but B and D are inconsistent with each other; therefore A and C are inconsistent.

The meaning no doubt is that where A is, B will be found, or that every A is a B, and similarly every C is a D; but B and D cannot occur together. The premises therefore appear to be of the form

$$A = AB, \qquad (1)$$

$$C = CD, \qquad (2)$$

$$B = Bd. \qquad (3)$$

On examining the series of sixteen combinations, but five are found to be consistent with the above conditions, namely,

$$\begin{array}{l} ABcd \\ aBcd \\ abCD \\ abcD \\ abcd. \end{array}$$

In these combinations the only A which appears is joined to *c*, and similarly C is joined to *a*, or A is inconsistent with C.

A more complex argument, also given by De Morgan[f], contains five terms, and is as stated below, except that I have altered the letters.

> 'Every A is one only of the two B or C; D is both B and C, except when B is E, and then it is neither; therefore no A is D.'

A little reflection will show that these premises are capable of expression in the following symbolic forms—

$$A = ABc + AbC, \qquad (1)$$

$$De = DeBC, \qquad (2)$$

$$DE = DEbc. \qquad (3)$$

As five letters, A, B, C, D, E, enter into these premises it is requisite to treat their thirty-two combinations, and it will be found that fourteen of them remain consistent with the premises, namely

AB*cd*E
AB*cde*
A*b*C*d*E
A*b*C*de*
*a*BCD*e*
*a*BC*d*E
*a*BC*de*
*a*B*cd*E
*a*B*cde*
*ab*C*d*E
*ab*C*de*
*abc*DE
*abcd*E
abcde.

Now if we examine the first four combinations, all of which contain A, we find that they none of them contain D; or again if we select those which contain D, we have only two, thus—

$$D = aBCDe + abcDE.$$

Hence it is clear that no A is D, and *vice versâ* no D is A. We might also draw many other conclusions from the premises; for instance—

$$DE = abcDE,$$

or D and E never meet but in the absence of A, B, and C.

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[f] 'Formal Logic,' p. 124.

Fallacies analysed by the Indirect Method.

It has been sufficiently shown, perhaps, that we can by the Indirect Method of Inference extract the whole truth from any series of propositions, and exhibit it anew in any required form of conclusion. But it may also need to be shown by examples that so long as we follow correctly the almost mechanical rules of the method, we cannot fall into any of the common fallacies or paralogisms which are not seldom committed in ordinary discussion. Let us take the example of a fallacious argument, previously treated by the Method of Direct Inference (p. 75),

Granite is not a sedimentary rock, (1)
Basalt is not a sedimentary rock, (2)

and let us ascertain whether any precise conclusion can be drawn concerning the relation of granite and basalt. Taking as before

A = granite,
B = sedimentary rock,
C = basalt,

the premises become $A = Ab$, (1)
$C = Cb$. (2)

Of the eight conceivable combinations of A, B, C, five agree with these conditions, namely

$$\begin{array}{ll} AbC & aBc \\ Abc & abC \\ & abc\,; \end{array}$$

the description of granite is found to be

$$A = AbC + Abc = Ab(C + c),$$

that is, granite is not a sedimentary rock but is either basalt or not-basalt. If we want a description of basalt the answer is of like form

$$C = AbC + abC = bC\,(A + a).$$

Basalt is a sedimentary rock, and either granite or not-granite. As it is already perfectly evident that basalt must be either granite or not, and *vice versâ*, the premises fail to give us any information on the point, that is to say the Method of Indirect Inference saves us from falling into any fallacious conclusions. This example sufficiently illustrates both the fallacy of Negative premises and that of Undistributed Middle of the old logic (pp. 75–77).

The fallacy called the Illicit Process of the Major Term is also incapable of commission in following the rules of the method. Our example was (p. 77)

All planets are subject to gravity, (1)
Fixed stars are not planets. (2)

The false conclusion is that 'fixed stars are not subject to gravity.' The terms are

A = planet
B = fixed star
C = subject to gravity.

And the premises are $A = AC$, (1)
$B = aB$. (2)

The combinations which remain uncontradicted on comparison with these premises are

$$\begin{array}{ll} AbC & aBc \\ aBC & abC \\ & abc. \end{array}$$

For fixed star we have the description

$$B = aBC + aBc,$$

that is, 'a fixed star is not a planet, but is either subject or not, as the case may be, to gravity.'

The Logical Abacus.

The Indirect Method of Inference has now been sufficiently described, and a careful examination of its powers will show that it is capable of giving a full analysis and solution of every question involving any simply logical relations. The chief difficulty of the method consists in the great number of combinations which may have to be examined; not only may the requisite labour become formidable, but a considerable chance of mistake may arise. I have therefore given much attention to modes of facilitating the work, and have succeeded in reducing the method to an almost mechanical form. It soon appeared obvious that if the conceivable combinations of the abecedarium, for any number of letters, instead of being printed in fixed order on a piece of paper or slate, were marked upon light moveable pieces of wood, mechanical arrangements could readily be devised for selecting the combinations in any required order. The labour of comparison and rejection might thus be immensely reduced. This idea was first carried out in the Logical Abacus, which I have found useful in the lecture-room for exhibiting the complete solution of logical problems. A minute description of the construction and use of the abacus, together with figures of the parts, has already been given in my essay called *The Substitution of Similars*[g], and I will here give only a general description.

The abacus consists of a common school black-board placed in a sloping position and furnished with four horizontal and equi-distant ledges. The combinations of the letters shown in the first four columns of the abecedarium (see p. 109), are printed in somewhat large type, so that each letter is about an inch from the neighbouring one, but the letters are placed one above the other instead of being in horizontal lines as in p. 109. Each combination of letters is separately fixed to the surface of a thin slip of wood one inch broad and about one-eighth inch thick. Short steel pins are then driven in an inclined position into the wood. When a letter is a large capital representing a positive term, the pin is fixed in the upper part of its space; when the letter is a small italic representing a negative term, the pin is fixed in the lower part of the space. Now, if one of the series of combinations be ranged upon a ledge of the black-board, the sharp edge of a flat rule can be inserted beneath the pins belonging to any one letter—say A, so that all the combinations marked A can be lifted out and placed upon a separate ledge. Thus we have represented the act of thought which separates the class A from what is not-A. The operation can be repeated; out of the A's we can in like manner select those which are B, obtaining the AB's; and in like manner we might select any other class such as the *a*B's, the *ab*'s or the *abc*'s.

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[g] Pp. 55-59, 81-86.

If now we take the series of eight combinations of the letters A, B, C, *a*, *b*, *c*, and wish to analyse the argument anciently called Barbara, having the premises

$$A = AB \qquad (1)$$

$$B = BC, \qquad (2)$$

we proceed as follows:—Firstly we raise the combinations marked *a*, leaving the A's behind; out of these A's we move to a lower ledge such as are not-B's, and to the remaining AB's we join the *a*'s which have been raised. The result is that we have divided all the combinations into two classes, namely, the A*b*'s which are incapable of existing consistently with premise (1), and the combinations which are consistent with the premise. Turning now to the second premise, we raise out of those which agree with (1) the *b*'s, then we lower the B*c*'s; lastly we join the *b*'s to the BC's. We should now find our combinations arranged as below.

A B C				*a* B C		*a* *b* C	*a* *b* *c*
	A B *c*	A *b* C	A *b* *c*		*a* B *c*		

The lower line contains all the combinations which are inconsistent with either premise; we have carried out in a mechanical manner that exclusion of self-contradictories which was formerly done upon the slate or paper. Accordingly, from the remaining combinations in the upper line we can draw any inference which the premises yield. If we raise the A's we find only one, and that is C, so that A must be C. If we select the *c*'s we again find only one which is *a* and also *b*, so that we prove that not-C is not-A and not-B.

When a disjunctive proposition occurs among the premises the requisite movements become rather more complicated. Take the disjunctive argument

A is either B or C or D,
A is not C and not D,
Therefore A is B.

The premises are represented accurately as follows:—

$$A = AB \cdot\!\!\!| \, AC \cdot\!\!\!| \, AD \qquad (1)$$

$$A = Ac \qquad (2)$$

$$A = Ad. \qquad (3)$$

As there are four terms we choose the series of sixteen combinations and place them on the highest ledge of the board but one. We raise the *a*'s and lower the *b*'s. But we are not to reject all the A*b*'s as contradictory, because by the first premise A's may be either B's or C's or D's. Accordingly out of the A*b*'s we must select the *c*'s, and out of these again the *d*'s, so that only A*bcd* will remain to be rejected finally. Joining all the other fifteen combinations together again we raise the *a*'s and lower the AC's, and thus reject the combinations inconsistent with (2); similarly we reject the AD's which are inconsistent with (3). It will be found that there remain in addition to all the eight combinations containing *a* only one containing A, namely

AB*cd*,

whence it is apparent that A must be B, the true conclusion of the argument.

In my previous Essay[h] I have described the working of two other logical problems upon the abacus, which it would be tedious to repeat in this place.

The Logical Machine.

Although the Logical Abacus considerably reduced the labour of using the Indirect Method, it was not free from the possibility of error. I thought moreover that it would afford a conspicuous proof of the generality and power of the method if I could reduce it to a purely mechanical form. Logicians had long been accustomed to speak of Logic as an Organon or Instrument, and even Bacon, while he rejected the old syllogistic logic, had insisted, in the second aphorism of his 'New Instrument,' that the mind required some kind of systematic aid. In the kindred science of mathematics mechanical assistance of one kind or another had long been employed. Orreries, globes, mechanical clocks, and such like instruments, are really aids to calculation and are of considerable antiquity. The arithmetical machine of Pascal is more than two centuries old, having been constructed in 1642–45. M. Thomas of Colmar has recently manufactured an arithmetical machine on Pascal's principles which is extensively employed by engineers and others who need frequently to multiply or divide. To Babbage, however, was entirely due the merit of embodying the Calculus of Differences in a machine, which thus became capable of calculating the most complicated tables of figures. It seemed strange that in the more intricate science of quantity mechanism could be applicable, whereas in the simple science of qualitative reasoning, the syllogism was only by analogy or simile called an Instrument. Swift satirically described the Professors of Laputa as in possession of a thinking machine, and in 1851 Mr. Alfred Smee actually proposed the construction of a Relational machine and a Differential machine, the first of which would be a mechanical dictionary and the second a mode of comparing ideas; but with these exceptions I have not yet met with so much as a suggestion of a reasoning machine. It may be added that Mr. Smee's designs, though highly ingenious, appear impracticable, and in any case do not attempt the performance of logical inference[i].

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[h] 'Substitution of Similars,' pp. 56–59.

The Logical Abacus soon suggested the notion of a Logical Machine, which, after two unsuccessful attempts, I succeeded in constructing in a comparatively simple and effective form. The details of the Logical Machine have been fully described by the aid of plates in the Philosophical Transactions[k], and it would be both tedious and needless to repeat the account of the somewhat intricate movements of the machine in this place.

The general appearance of the machine is shown in a plate facing the title-page of this volume. It somewhat resembles a very small upright piano or organ, and has a keyboard containing twenty-one keys. These keys are of two kinds, sixteen of them representing the terms or letters A, *a*, B, *b*, C, *c*, D, *d*, which have so often been employed in our logical notation. When letters occur on the left-hand side of a proposition, formerly called the subject, each is represented by a key on the left-hand half of the keyboard; but when they occur on the right-hand side, or as it used to be called the predicate of the proposition, the letter keys on the right-hand side of the keyboard are the proper representatives. The five other keys may be called operation keys, to distinguish them from the letter or term keys. They stand for the stops, copula, and disjunctive conjunctions of a proposition. The middle key of all is the copula, to be pressed when the verb *is* or the sign = is met. The extreme right-hand key is called the Full Stop, because it should be pressed when a proposition is completed, in fact in the proper place of the full stop. The extreme left-hand key is used to terminate an argument or to restore the machine to its initial condition; it is called the Finis key. The last key but one on the right and left complete the whole series, and represent the conjunction *or* in its unexclusive meaning, or the sign + which I have employed, according as it occurs in the right or left hand side of the proposition. The whole keyboard is arranged as shown below—

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[i] See his work called 'The Process of Thought adapted to Words and Language, together with a description of the Relational and Differential Machines.' Also 'Philosophical Transactions,' [1870] vol. 160, p. 518.

[k] 'Philosophical Transactions,' [1870] vol. 160, p. 497. 'Proceedings of the Royal Society,' vol. xviii. p. 166, Jan. 20, 1870. 'Nature,' vol. i. p. 343.

Finis	Left-hand side of Proposition, or Subject.									Copula.	Right-hand side of Proposition, or Predicate.									Full Stop.
	+ Or	*d*	D	*c*	C	*b*	B	*a*	A		A	*a*	B	*b*	C	*c*	D	*d*	+ Or	

To work the machine it is only requisite to press the keys in succession as indicated by the letters and signs of a symbolical proposition. All the premises of an argument are supposed to be reduced to the simple notation which has been employed in the previous pages. Taking then such a simple proposition as

$$A = AB,$$

we press the keys A (subject), copula, A (predicate), B (predicate), and full stop.

If there be a second premise, for instance

$$B = BC,$$

we press in like manner the keys—

B (subj.), copula, B (pred.), C (pred.), full stop.

The process is exactly the same however numerous the premises may be. When they are completed the operator will see indicated on the face of the machine the exact combinations of letters which are consistent with the premises according to the principles of thought.

As shown in the figure opposite the title-page, the machine exhibits in front an abecedarium of sixteen combinations, exactly like that of the abacus, except that the letters of each combination are separated by a certain interval. After the above problem has been worked upon the machine the abecedarium will present the following appearance—

A	A							*a*	*a*			*a*	*a*	*a*	*a*
B	B							B	B			*b*	*b*	*b*	*b*
C	C							C	C			C	C	*c*	*c*
D	*d*							D	*d*			D	*d*	D	*d*

The operator will collect the various conclusions, as for instance that A is always C, that not-C is not-B and not-A; that not-B is not-A but either C or not-C, as in the use of the Logical Slate or Abacus.

Disjunctive propositions are to be treated in an exactly similar manner. Thus, to work the premises

$$A = AB \mathbin{\cdot\!|\!\cdot} AC$$
$$B \mathbin{\cdot\!|\!\cdot} C = BD \mathbin{\cdot\!|\!\cdot} CD,$$

it is only necessary to press in succession the keys

A (subj.), copula, A (pred.), B, ꖌ A,C, full stop.
B (subj.), ꖌ C, copula, B (pred.), D, ꖌ, C, D, full stop.

The combinations then remaining will be as follows

ABCD *a*BCD
ABcD *a*B*c*D
A*b*CD *ab*CD
*abc*D
abcd.

On pressing the subject key A, all the possible combinations which do not contain A will disappear, and the description of A may be gathered from what remains, namely that it is always D. The full-stop key restores all combinations consistent with the premises and any other selection may be made, as say not-D, which will be found to be always not-A, not-B, and not-C.

At the end of every problem, when no further questions need be addressed to the machine, it is desirable to press the Finis key, which has the effect of bringing into view the whole of the conceivable combinations of the abecedarium. This key in fact obliterates the conditions impressed upon the machine by moving back into their ordinary places those combinations which had been rejected as inconsistent with the premises. Before beginning any new problem it is requisite to observe that the whole sixteen combinations are visible. After the Finis key has been used the machine represents a mind endowed with powers of thought, but wholly devoid of knowledge. It would not in that condition give any answer but such as would consist in the primary laws of thought themselves. But when any proposition is worked upon the keys, the machine analyses or digests the meaning of it and becomes charged with the knowledge embodied in that proposition. Accordingly it is able to return as an answer any description of a term or class so far as furnished by that proposition in accordance with the Laws of Thought. The machine is thus the embodiment of a true logical system. The combinations are classified, selected or rejected just as they should be by a reasoning mind, so that at each step in a problem, the abecedarium represents the proper condition of a mind exempt from mistake. It cannot be asserted indeed that the machine entirely supersedes the agency of conscious thought; mental labour is required in interpreting the meaning of grammatical expressions and in correctly impressing that meaning on the machine; it is further required in gathering the conclusion from the remaining combinations. Nevertheless the true process of logical inference is actually accomplished in a purely mechanical manner.

It is worthy of remark that the machine can detect any self-contradiction existing between the premises presented to it, for it will then be found that one or more of the terms disappear entirely from the abecedarium. Thus if we worked the two propositions, A is B, and A is not-B, and then inquired for a description of A, the machine would refuse to give it by exhibiting no combination at all containing A. This result is in agreement with the law which I have explained that every term must have its negative (p. 88). Accordingly whenever any one of the letters A, B, C, D, *a*, *b*, *c*, *d* wholly disappears from the abecedarium, it may be safely inferred that some self-contradiction has been committed in the premises.

It ought to be carefully observed that the logical machine cannot receive a simple identity of the form

A = B except in the double form of A = AB and B = AB. To work the proposition A = B it is therefore necessary to press the keys—A (subj.), Copula, A (pred.), B (pred.), Full stop, B (subj.), Copula, A (pred.), B (pred.), Full stop. The same double operation will be necessary whenever the proposition is not of the kind called a partial identity (p. 47). Thus AB = CD, AB = AC, A = B ꖌ C, A ꖌ B = C ꖌ D, all require to be read from both ends separately. This is a remarkable fact which some persons may consider as militating against the equational form of proposition, but I do not think this is really the case.

Before leaving the subject I may remark that these mechanical devices are not likely to possess great practical utility. We do not require in common life to be constantly solving complex logical questions. Even in mathematical calculation the ordinary rules of arithmetic are generally sufficient, and a calculating machine could only be used with advantage in peculiar cases. But the machine and abacus have nevertheless two important uses.

1. I trust that the time is not very far distant when the predominance of the ancient Aristotelian Logic will be a matter of history, and the teaching of logic will be placed on a footing more worthy of its supreme importance. It will then be found that the solution of logical questions is an exercise of mind at least as valuable and necessary as mathematical calculation. I believe that these mechanical devices, or something of the same kind, will then become useful for exhibiting to a class of students a clear and visible analysis of logical problems of any degree of complexity, the nature of each step being rendered plain to the eye. For this purpose I have already often used the machine or abacus in my class lectures at the Owens College.

2. The more immediate importance of the machine seems to consist in the unquestionable proof which it affords that most comprehensive views of the principles of reasoning have now been attained, although they were almost wholly unknown to Aristotle and his followers. The time must come when the inevitable results of the admirable writings of the late Dr. Boole must be recognised at their true value, and the plain and palpable form in which the machine presents those results will, I hope, hasten the time. Undoubtedly his life marks an era in the high science of human reason. It may seem strange that it had remained for him first to set forth in its full extent the problem of logic, but I am not aware that any one before him had treated logic as a symbolic method for evolving from any premises the description of any class whatsoever as defined by those premises. His quasi-mathematical system indeed could not be regarded as a final and complete solution of the problem. Not only did it require the manipulation of mathematical symbols in a very intricate and perplexing manner, but the results when obtained were devoid of demonstrative force, because they turned upon the employment of unintelligible symbols, acquiring meaning only by analogy. I have also pointed out that he imported into his system a condition concerning the exclusive nature of alternatives (p. 83), which is not necessarily true of logical terms. I shall have to show in the next chapter that logic is really the basis of the whole science of mathematical reasoning, so that Boole completely inverted the true order of proof when he proposed to infer logical truths by algebraic processes. It is a wonderful evidence of his mental power that by methods fundamentally false he should have succeeded in reaching true conclusions and widening the sphere of reason.

The mechanical performance of logical inference affords a demonstration both of the truth of Boole's results and of the mistaken nature of his mode of deducing them. Conclusions which he could only obtain by pages of intricate calculation, are exhibited by the machine after one or two minutes of manipulation. And not only are those conclusions easily reached, but they are demonstratively true, because every step of the process involves nothing more obscure that the Laws of Thought.

The Order of Premises.

Before quitting the subject of deductive reasoning, I may remark that the order in which the premises of an argument, or any propositions whatsoever, are placed, is a matter of logical indifference. Much discussion has taken place at various times concerning the arrangement of the premises of a syllogism; and it has been generally held, in accordance with the opinion of Aristotle, that the so-called major premise, containing the major term, or the predicate of the conclusion, should stand first. This distinction however falls to the ground in our system, since the proposition is reduced to an identical form in which there is no distinction of subject and predicate. In a strictly logical and philosophic point of view the order of statement is wholly devoid of significance. The premises are simultaneously coexistent, and are not related to each other according to any of the properties of space or time. Just as the qualities of the same object are neither before nor after each other in nature (p. 40), and are only thought of in some one order owing to our limited capacity of mind, so the premises of an argument are neither before nor after each other, and are only thought of in succession because the mind cannot grasp many ideas at once. The logical combinations of the Abecedarium are exactly the same in whatever order the premises be treated on the logical slate or machine.

Some difference may doubtless exist as regards convenience to human memory. The mind may take in the results of an argument more easily in one mode of statement than another, although there is no real difference in the logical results. But in this point of view I think that Aristotle and the old logicians were clearly wrong. It is more easy to conclude that 'all A's are C's' from 'all A's are B's and all B's are C's,' than from the same propositions in inverted order, 'all B's are C's and all A's are B's.'

The Equivalency of Propositions.

One great advantage which arises from the study of this Indirect Method of Inference consists in the clear notion which we thus gain of the Equivalency of Propositions. The older logicians showed how from certain simple premises we might draw an inference, but they failed to point out whether that inference contained the whole, or only a part, of the information embodied in the premises. Now any one proposition or group of propositions may be classed with respect to another proposition or group of propositions, as

1. Equivalent,
2. Inferrible,
3. Consistent,
4. Contradictory.

Taking the proposition 'All men are mortals' as the original, 'All immortals are not men' is its equivalent; 'Some mortals are men' is inferrible, or capable of inference, but is not equivalent; 'All not men are not mortals' cannot be inferred, but is consistent, that is, may be true at the same time; 'All men are immortals' is of course contradictory.

One sufficient test of equivalency is the capability of mutual inference. Thus from

All electrics = all non-conductors,

I can infer

All non-electrics = all conductors,

and *vice versâ* from the latter I can pass back to the former. In short $A = B$ is equivalent to $a = b$. Again, from the union of the two propositions, $A = AB$ and $B = AB$, I get $A = B$, and from this I might as easily deduce the two with which I started. In this case one proposition is equivalent to two other propositions. There are indeed no less than four modes in which we may express the identity of two classes A and B, namely,

FIRST MODE.	SECOND MODE.	THIRD MODE.	FOURTH MODE.
$A = B$	$a = b$	$\left.\begin{array}{l}A = AB \\ B = AB\end{array}\right\}$	$\left.\begin{array}{l}a = ab \\ b = ab\end{array}\right\}$

The Indirect Method of Inference furnishes an universal and clear criterion as to the relationship of propositions. The import of a statement is always to be measured by the combinations of terms which it destroys. Hence two propositions are exactly equivalent when they remove exactly the same combinations from the Abecedarium, and neither more nor less. A proposition is inferrible but not equivalent to another when it removes some but not all the combinations which the other removes. Again, propositions are consistent provided that they leave some one combination containing each term, and the negative of each term. If after all the combinations inconsistent with two propositions are struck out, there still appears in the Abecedarium each of the letters A, *a*, B, *b*, C, *c*, D, *d*, which were there before, then no inconsistency between the propositions exists, although they may not be equivalent or even inferrible. Finally, contradictory propositions are those which altogether remove any one or more letter-terms from the Abecedarium.

What is true of single propositions applies also to groups of propositions, however large or complicated; that is to say, one group may be equivalent, inferrible, consistent, or contradictory as regards another, and we may similarly compare one proposition with a group of propositions.

To give in this place illustrations of all the four kinds of relation would require much space: as the examples given in previous sections or chapters may serve more or less to explain the relations of inference, consistency, and contradiction, I will only add a few instances of equivalent propositions or groups.

In the following list each proposition or group of propositions is exactly equivalent in meaning to the corresponding one in the other column, and the truth of this statement may be tested by working out the combinations of the Abecedarium, which ought to be found exactly the same in the case of each pair of equivalents.

$$
\begin{array}{ll}
A = Ab & B = aB \\
A = b & a = B \\
A = BC & a = b \mathbin{\cdot|\cdot} c \\
A = AB \mathbin{\cdot|\cdot} AC & b = ab \mathbin{\cdot|\cdot} AbC \\
A \mathbin{\cdot|\cdot} B = C \mathbin{\cdot|\cdot} D & ab = cd \\
A \mathbin{\cdot|\cdot} c = B \mathbin{\cdot|\cdot} d & aC = bD \\
A = ABc \mathbin{\cdot|\cdot} AbC & \left.\begin{array}{r} A = AB \mathbin{\cdot|\cdot} AC \\ AB = ABc \end{array}\right\} \\
\left.\begin{array}{l} A = B \\ B = C \end{array}\right\} & \left.\begin{array}{l} A = B \\ A = C \end{array}\right\} \\
\left.\begin{array}{l} A = AB \\ B = BC \end{array}\right\} & \left.\begin{array}{l} A = AC \\ B = A \mathbin{\cdot|\cdot} aBC \end{array}\right\} \\
\left.\begin{array}{l} A = AB \\ A = AC \\ A = AD \end{array}\right\} & A = ABCD.
\end{array}
$$

Although in these and many other cases the equivalents of certain propositions can readily be given, yet I believe that no uniform and infallible process can be pointed out by which the exact equivalents of premises can be ascertained. Ordinary deductive inference usually gives us only a portion of the contained information. It is true that the combinations consistent with a set of propositions are logically equivalent to them, but the difficulty consists in passing back from the combinations to a new set of propositions. The task is here of a different character from any which we have yet attempted. It is in reality an inverse process, and is just as much more troublesome and uncertain than the direct process, as seeking is compared with hiding. Not only may several different answers equally apply, but there is no method of discovering any of those answers except by repeated trial. The problem which we have here met is really that of induction, the inverse of deduction; and, as I shall soon show, induction is always tentative, and unless conducted with peculiar skill and insight must be exceedingly laborious in cases of any considerable complexity.

The late Professor de Morgan was unfortunately led by this equivalency of propositions into the most serious error of his ingenious system of Logic. He held that because the proposition 'All A's are all B's,' was but another expression for the two propositions 'All A's are B's' and 'All B's are A's,' it must be a composite and not really an elementary form of proposition[1]. But on taking a general view of the equivalency of propositions such an objection seems to have no weight. Logicians have, with few exceptions, persistently upheld the original error of Aristotle in rejecting from their science the one simple relation of identity on which all more complex logical relations must really rest.

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[1] 'Syllabus of a proposed system of Logic,' §§ 57, 121, &c. 'Formal Logic,' p. 66.

The Nature of Inference.

The question, What is Inference? is involved, even to the present day, in as much uncertainty as that ancient question, What is Truth? I shall in more than one part of this work endeavour to show that inference never does more than explicate, unfold, or develop the information contained in certain premises or facts. Neither in deductive nor inductive reasoning can we add a tittle to our implicit knowledge, which is like that contained in an unread book or a sealed letter. Sir W. Hamilton has well said, 'Reasoning is the showing out explicitly that a proposition not granted or supposed, is implicitly contained in something different which is granted or supposed[m].'

Professor Bowen has explained[n] with much clearness that the conclusion of an argument states explicitly what is virtually or implicitly thought. 'The process of reasoning is not so much a mode of evolving a new truth, as it is of establishing or proving an old one, by showing how much was admitted in the concession of the two premises taken together.' It is true that the whole meaning of these statements rests upon that of such words as 'explicit,' 'implicit,' 'virtual.' That is implicit which is wrapped up, and we render it explicit when we unfold it. Just as the conception of a circle involves a hundred important geometrical properties, all following from what we know, if we have acuteness to unfold the results, so every fact and statement involves more meaning than seems at first sight. Reasoning explicates or brings to conscious possession what was before unconscious. It does not create, nor does it destroy, but it transmutes and throws the same matter into a new form.

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[m] Lectures on Metaphysics, vol. iv. p. 369.

[n] Bowen, 'Treatise on Logic,' Cambridge, U. S., 1866; p. 362.

The difficult question still remains, Where does novelty of form begin? Is it a case of inference when we pass from 'Sincerity is the parent of truth' to 'The parent of truth is sincerity?' The old logicians would have called this change conversion, one case of immediate inference. But as all identity is necessarily reciprocal, and the very meaning of such a proposition is that the two terms are identical in their signification, I fail to see any difference between the statements whatever. As well might we say that $a = b$ and $b = a$ are different equations.

Another point of difficulty is to decide when a change is merely grammatical and when it involves a real logical transformation. Between a *table of wood* and a *wooden table* there is no logical difference (p. 37), the adjective being merely a convenient substitute for the prepositional phrase. But it is uncertain to my mind whether the change from 'All men are mortal' to 'No men are not mortal' is purely grammatical. Logical change may perhaps be best described as consisting in the determination of a relation between certain classes of objects from a relation between certain other classes. Thus I consider it a truly logical inference when we pass from 'All men are mortal' to 'All immortals are not-men,' because the classes *immortals* and *not-men* are different from *mortals* and *men*, and yet the propositions contain at the bottom the very same truth, as shown in the combinations of the Abecedarium.

From logical inference we must discriminate the passage from the qualitative to the quantitative form of a proposition. We state the same truth when we say that 'mortality belongs to all men,' as when we assert that 'all men are mortals.' Here we do not pass from class to class, but from one kind of term, the abstract, to another kind, the concrete. But inference probably enters when we pass from either of the above propositions to the assertion that the class of immortal men is zero, or contains no objects.

It is really a question of words to what processes we shall or shall not apply the name 'inference,' and I have no wish to continue the trifling discussions which have already taken place upon the subject. We shall not commit any serious error, provided that we always bear in mind that two propositions may be connected together in four different ways. They may be—

1. *Tautologous* or *identical*, involving the same relation between the same terms and classes, and only differing in the order of statement; thus 'Victoria is the Queen of England' is tautologous with 'The Queen of England is Victoria.'

2. *Grammatically equivalent*, in which the classes or objects are the same and similarly related, and the only difference is in the words; thus 'Victoria is the Queen of England' is grammatically equivalent to 'Victoria is England's Queen.'

3. Equivalent in qualitative and quantitative form, the classes being the same, but viewed in a different manner.

4. *Logically equivalent*, when the classes and relations are different, but involve the same knowledge of the possible combinations.

CHAPTER VII.

INDUCTION.

We enter in this chapter upon the second great department of logical method, that of Induction or the Inference of general from particular truths. It cannot be said that the Inductive process is of greater importance than the Deductive process already considered, because the latter process is absolutely essential to the existence of the former. Each is the complement and counterpart of the other. The principles of thought and existence which underlie them are at the bottom the same, just as subtraction of numbers necessarily rests upon the same principles as addition. Induction is, in fact, the inverse operation to deduction, and cannot be conceived to exist without the corresponding operation, so that the question of relative importance cannot arise. Who thinks of asking whether addition or subtraction is the more important process in arithmetic? But at the same time much difference in difficulty may exist between a direct and inverse operation; the integral calculus, for instance, is almost infinitely more difficult than the differential calculus of which it is the inverse. It must be allowed that in logic inductive investigations are of a far higher degree of difficulty, variety, and complexity than any questions of deduction; and it is this fact no doubt which has led some logicians to erroneous opinions concerning the exclusive importance of induction.

Hitherto we have been engaged in considering how from certain conditions, laws, or identities governing the combinations of qualities, we may deduce the nature of the combinations agreeing with those conditions. Our work has been to unfold the results of what is contained in any statements, and the process has been one of *Synthesis.* The terms or combinations of which the character has been determined have usually, though by no means always, involved more qualities, and therefore, by the relation of extension and intension, fewer objects than the terms in which they were described. The truths inferred were thus usually less general than the truths from which they were inferred.

In induction all is inverted. The truths to be ascertained are more general than the data from which they are drawn. The process by which they are reached is *analytical,* and consists in separating the complex combinations in which natural phenomena are presented to us, and determining the relations of separate qualities. Given events obeying certain unknown laws, we have to discover the laws obeyed. Instead of the comparatively easy task of finding what effects will follow from a given law, the effects are now given and the law is required. We have to interpret the will by which the conditions of creation were laid down.

Induction an Inverse Operation.

I have already asserted that induction is the inverse operation of deduction, but the difference is one of such great importance that I must dwell upon it. There are many cases where we can easily and infallibly do a certain thing but may have much trouble in undoing it. A person may walk into the most complicated labyrinth or the most extensive catacombs, and turn hither and thither at his will; it is when he wishes to return that doubt and difficulty commence. In entering, any path served him; in leaving, he must select certain definite paths, and in this selection he must either trust to memory of the way he entered or else make an exhaustive trial of all possible ways. The explorer entering a new country makes sure his line of return by barking the trees.

The same difficulty arises in many scientific processes. Given any two numbers, we may by a simple and infallible process obtain their product, but it is quite another matter when a large number is given to determine its factors. Can the reader say what two numbers multiplied together will produce the number 8,616,460,799? I think it unlikely that any one but myself will ever know; for they are two large prime numbers, and can only be rediscovered by trying in succession a long series of prime divisors until the right one be fallen upon. The work would probably occupy a good computer for many weeks, but it did not occupy me many minutes to multiply the two factors together. Similarly there is no direct process for discovering whether any number is a prime or not; it is only by exhaustingly trying all inferior numbers which could be divisors, that we can show there is none, and the labour of the process would be intolerable were it not performed systematically once for all in the process known as the Sieve of Eratosthenes, the results being registered in tables of prime numbers.

The immense difficulties which are encountered in the solution of algebraic equations are another illustration. Given any algebraic factors, we can easily and infallibly arrive at the product, but given a product it is a matter of infinite difficulty to resolve it into factors. Given any series of quantities however numerous, there is very little trouble in making an equation which shall have those quantities as roots. Let a, b, c, d, &c., be the quantities; then

$$(x-a)\,(x-b)\,(x-c)\,(x-d)\ldots\ldots=0$$

is the equation required, and we only need to multiply out the expression on the left hand by ordinary rules. But having given a complex algebraic expression equated to *zero*, it is a matter of exceeding difficulty to discover all the roots. Mathematicians have exhausted their highest powers in carrying the complete solution up to the fourth degree. In every other mathematical operation the inverse process is far more difficult than the direct process, subtraction than addition, division than multiplication, evolution than involution; but the difficulty increases vastly as the process becomes more complex. The differentiation, the direct process, is always capable of performance by certain fixed rules, but as these produce considerable variety of results, the inverse process of integration presents immense difficulties, and in an infinite majority of cases surpasses the present resources of mathematicians. There are no infallible and general rules for its accomplishment; it must be done by trial, by guesswork, by remembering the results of differentiation, and using them as a guide.

Coming more nearly to our own immediate subject, exactly the same difficulty exists in determining the law which certain numbers obey. Given a general mathematical expression, we can infallibly ascertain its value for any required value of the variable. But I am not aware that mathematicians have ever attempted to lay down the rules of a process by which, having given certain numbers, one might discover a rational or precise formula from which they proceed. The problem is always indeterminate, because an infinite number of formulæ agreeing with certain numbers, might always be discovered with sufficient trouble.

The reader may test his power of detecting a law, by contemplation of its results, if he, not being a mathematician, will attempt to point out the law obeyed by the following numbers:

$$-\frac{1}{2}, \frac{1}{6}, -\frac{1}{30}, \frac{1}{42}, -\frac{1}{30}, \frac{5}{66}, \frac{691}{2730}, \frac{7}{6}, \frac{3617}{510}, \text{etc.}$$

These numbers are sometimes negative, more often positive; sometimes in low terms, but unexpectedly springing up to high terms; in absolute magnitude they are very variable. They seem to set all regularity and method at defiance, and it is hardly to be supposed that any one could, from contemplation of the numbers, have detected the relation between them. Yet they are derived from the most regular and symmetrical laws of relation, and are of the highest importance in mathematical analysis, being known as the numbers of Bernouilli.

Compare again the difficulty of decyphering with that of cyphering. Any one can invent a secret language, and with a little steady labour can translate the longest letter into the character. But to decypher the letter having no key to the signs adopted, is a wholly different matter. As the possible modes of secret writing are infinite in number and exceedingly various in kind, there is no direct mode of discovery whatever. Repeated trial, guided more or less by knowledge of the customary form of cypher, and resting entirely on the principles of probability, is the only resource. A peculiar tact or skill is requisite for the process, and a few men, such as Wallis or Mr. Wheatstone, have attained great success.

Induction is the decyphering of the hidden meaning of natural phenomena. Given events which happen in certain definite combinations, we are required to point out the laws which have governed those combinations. Any laws being supposed, we can, with ease and certainty, decide whether the phenomena obey those laws. But the laws which may exist are infinite in variety, so that the chances are immensely against mere random guessing. The difficulty is much increased by the fact that several laws will usually be in operation at the same time, the effects of which are complicated together. The only modes of discovery consist either in exhaustively trying a great number of supposed laws, a process which is exhaustive in more senses than one, or else by carefully contemplating the effects, endeavouring to remember cases in which like effects followed from known laws. However we accomplish the discovery, it must be done by the more or less apparent application of the direct process of deduction.

The Logical Abecedarium illustrates induction as well as it does deduction. In the Indirect process of Inference we found that from certain propositions we could infallibly determine the combinations of terms agreeing with those premises. The inductive problem is just the inverse. Having given certain combinations of terms, we need to ascertain the propositions with which they are consistent, and from which they may have proceeded. Now if the reader contemplates the following combinations

ABC   *ab*C
*a*BC   *abc*,

he will probably remember at once that they belong to the premises A = AB, B = BC. If not, he will require a few trials before he meets with the right answer, and every trial will consist in assuming certain laws and observing whether the deduced results agree with the data. To test the facility with which he can solve this inductive problem, let him casually strike out any of the combinations, say of the fourth column of the Abecedarium (p. 109), and say what laws the remaining combinations obey, observing that every one of the letter-terms and their negatives ought to appear in order to avoid self-contradiction in the premises (pp. 88, 128). Let him say, for instance, what laws are embodied in the combinations

ABC   *a*BC
A*bc*   *ab*C.

The difficulty becomes much greater when more terms enter into the combinations. It would be no easy matter to point out the complete conditions fulfilled in the combinations

$\mathrm{AC}e$
$a\mathrm{BC}e$
$a\mathrm{B}cd\mathrm{E}$
$ab\mathrm{C}e$
$abc\mathrm{E}$.

After some trouble the reader may discover that the principal laws are $\mathrm{C} = e$, and $\mathrm{A} = \mathrm{A}e$; but he would hardly discover the remaining law, namely that $\mathrm{BD} = \mathrm{BD}e$.

The difficulties encountered in the inductive investigations of nature, are of an exactly similar kind.

We seldom observe any great law in uninterrupted and undisguised operation. The acuteness of Aristotle and the ancient Greeks, did not enable them to detect that all terrestrial bodies tend to fall towards the centre of the earth. A very few nights of observation would have convinced an astronomer viewing the solar system from its centre, that the planets travelled round the sun; but the fact that our place of observation is one of the travelling planets, so complicates the apparent motions of the other bodies, that it required all the industry and sagacity of Copernicus to prove the real simplicity of the planetary system. It is the same throughout nature; the laws may be simple, but their combined effects are not simple, and we have no clue to guide us through their intricacies. 'It is the glory of God,' said Solomon, 'to conceal a thing, but the glory of a king to search it out.' The laws of nature are the invaluable secrets which God has hidden, and it is the kingly prerogative of the philosopher to search them out by industry and sagacity.

Induction of Simple Identities.

Many of the most important laws of nature are expressible in the form of simple identities, and I can at once adduce them as examples to illustrate what I have said of the difficulty of the inverse process of induction. There are many cases in which two phenomena are usually conjoined. Thus all gravitating matter is exactly coincident with all matter possessing inertia ; where one property appears, the other likewise appears. All crystals of the cubical system, are all the crystals which do not doubly refract light. All exogenous plants are, with some exceptions, those which have two cotyledons or seed-leaves.

A little reflection will show that there is no direct and infallible process by which such complete coincidences may be discovered. Natural objects are aggregates of many qualities, and any one of those qualities may prove to be in close connection with some others. If each of a numerous group of objects is endowed with a hundred distinct physical or chemical qualities, there will be no less than $\frac{1}{2}(100 \times 99)$ or 4950 pairs of qualities, which may be connected, and it will evidently be a matter of great intricacy and labour to ascertain exactly which qualities are connected by any simple law.

One principal source of difficulty is that the finite powers of the human mind are not sufficient to compare by a single act any large group of objects with another large group. We cannot hold in the conscious possession of the mind at any one moment more than five or six different ideas. Hence we must treat any more complex group by successive acts of attention. The reader will perceive by an almost individual act of comparison that the words *Roma* and *Mora* contain the same letters. He may perhaps see at a glance whether the same is true of *Causal* and *Casual*, and of *Logica* and *Caligo*. To assure himself that the letters in *Astronomers* make *No more stars*, that *Serpens in akuleo* is an anagram of *Joannes Keplerus*, or *Great gun do us a sum* an anagram of *Augustus de Morgan*, it will certainly be necessary to break up the act of comparison into several successive acts. The process will acquire a double character, and will consist in ascertaining that each letter of the first group is among the letters of the second group, and *vice versâ*, that each letter of the second is among those of the first group. In the same way we can only prove that two long lists of names are identical, by showing that each name in one list occurs in the other, and *vice versâ*.

This process of comparison really consists in establishing two partial identities, which are, as already shown (p. 133), equivalent in conjunction to one simple identity. We first ascertain the truth of the two propositions A = AB, B = AB, and we then rise by substitution to the single law A = B.

There is another process, it is true, by which we may get to exactly the same result, for the two propositions A = AB, $a = ab$ are also equivalent to the simple identity A = B (p. 133). If then we can show that all objects included under A are included under B, and also that all objects not included under A are not included under B, our purpose is effected. By this process we should usually compare two lists if we are allowed to mark them. For each name in the first list we should strike off one in the second, and if, when the first list is exhausted the second list is also exhausted, it follows that all names absent from the first must be absent from the second, and the coincidence must be complete.

The two modes of proving a simple identity are so closely allied that it is doubtful how far we can detect any difference in their powers and instances of application. The first method is perhaps more convenient where the phenomena to be compared are rare. Thus we prove that all the musical concords coincide with all the more simple numerical ratios, by showing that each concord arises from a simple ratio of undulations, and then showing that each simple ratio gives rise to one of the concords. To examine all the possible cases of discord or complex ratio of undulation would be impossible. By a happy stroke of induction Sir John Herschel discovered that all crystals of quartz which rotate the plane of polarization of light are precisely those crystals which have plagihedral faces, that is, oblique faces on the corners of the prism unsymmetrical with the ordinary faces. This singular relation would be proved by observing that all plagihedral crystals possessed the power of rotation, and *vice versâ* all crystals possessing this power were plagihedral. But it might at the same time be noticed that all ordinary crystals were devoid of the power. There is no reason why we should not observe any of the four propositions $A = AB$, $B = AB$, $a = ab$, $b = ab$, all of which follow from $A = B$ (see p. 133).

Sometimes the terms of the identity may be singular objects; thus we observe that diamond is a combustible gem, and being unable to discover any other that is, we affirm

Diamond = combustible gem.

In a similar manner we ascertain that

Mercury = metal liquid at ordinary temperatures,
Substance of least density = substance of least atomic weight.

Two or three objects may occasionally enter into the induction, as when we learn that

Sodium + potassium = metal of less density than water,
Venus + Mercury + Mars = major planet devoid of satellites.

Induction of Partial Identities.

We found in the last section that the simple identity of two classes is almost always discovered not by direct observation of the fact, but by first establishing two partial identities. There are also a great multitude of cases in which the partial identity of one class with another is the only relation to be discovered. Thus the most common of all inductive inferences consists in establishing the fact that all objects having the properties of A have also those of B, or that A = AB. To ascertain the truth of a proposition of this kind it is merely necessary to assemble together, mentally or physically, all the objects included under A, and then observe whether B is present in each of them, or, which is the same, whether it would be impossible to select from among them any not-B. Thus, if we mentally assemble together all the heavenly bodies which move with apparent rapidity, that is to say the planets, we find that they all possess the property of not scintillating. We cannot analyse any vegetable substance without discovering that it contains carbon and hydrogen, but it is not true that all substances containing carbon and hydrogen are vegetable substances.

The great mass of scientific truths consists of propositions of this form A = AB. Thus in astronomy we learn that all the planets are spheroidal bodies; that they all revolve in one direction round the sun; that they all shine by reflected light; that they all obey the law of gravitation. But of course it is not to be asserted that all bodies obeying the law of gravitation, or shining by reflected light, or revolving in a particular direction, or being spheroidal in form, are planets. In other sciences we have immense numbers of propositions of the same form, as for instance that all substances in becoming gaseous absorb heat; that all metals are elements; that they are all good conductors of heat and electricity; that all the alkaline metals are monad elements; that all foraminifera are marine organisms; that all parasitic animals are non-mammalian; that lightning never issues from stratous clouds[a]; that pumice never occurs where only Labrador felspar is present[b]: and scientific importance may attach even to such apparently trifling observations as that 'white cats having blue eyes are deaf[c].'

The process of inference by which all such truths are obtained may readily be exhibited in a precise symbolic form. We must have one premise specifying in a disjunctive form all the possible individuals which belong to a class; we resolve the class, in short, into its constituents. We then need a number of propositions each of which affirms that one of the individuals possesses a certain property. Thus the premises must be of the form

$$\begin{aligned} A &= B + C + D + \ldots\ldots + P + Q \\ B &= BX \\ C &= CX \\ \ldots &\quad \ldots \\ \ldots &\quad \ldots \\ Q &= QX. \end{aligned}$$

Now if we substitute for each alternative of the first premise its description as found among the succeeding premises we obtain

$$A = BX + CX + \ldots\ldots\ldots + PX + QX$$

or

$$A = (B + C + \ldots\ldots + Q)X.$$

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[a] Arago's Meteorological Essays, p. 10.

[b] Lyell's Elements of Geology, Fourth ed. p. 373.

[c] Darwin's Variation of Animals, &c.

But for the aggregate of alternatives we may now substitute their equivalent as given in the first premise, namely A, so that we get the required result

$$A = AX.$$

It may be remarked that we should have reached the same final result if our original premise had been of the form

$$A = AB + AC + \ldots\ldots + AQ.$$

The difference of meaning is that all B's need not now be A's, nor all C's, &c. But we should still have

$$A = ABX + ACX + \ldots\ldots + AQX = AX.$$

We can always prove a proposition, if we find it more convenient, by proving its equivalent. To assert that all not-B's are not-A's, is exactly the same as to assert that all A's are B's. Accordingly we may ascertain that $A = AB$ by first ascertaining that $b = ab$. If we observe, for instance, that all substances which are not solids are also not capable of double refraction, it follows necessarily that all double refracting substances are solids. We may convince ourselves that all electric substances are nonconductors of electricity, by reflecting that all good conductors do not, and in fact cannot, retain electric excitation. When we come to questions of probability it will be found desirable to prove, as far as possible, both the original proposition and its equivalent, as there is then an increased area of observation.

The number of alternatives which may arise in the division of a class varies greatly, and may be any number from two upwards. Thus it is probable that every substance is either magnetic or diamagnetic, and no substance can be both at the same time. The division then must be made in the form

$$A = ABc + AbC.$$

If now we can prove that all magnetic substances are capable of polarity, say $B = BC$, and also that all diamagnetic substances are capable of polarity C = CD, it follows by substitution that all substances are capable of polarity, or A = AD. We may divide the class substance again into the three subclasses, solid, liquid, and gas; and if we can show that in each of these forms it obeys Carnot's thermodynamic law, it follows that all substances obey that law. Similarly we may show that all vertebrate animals possess red blood, if we can show separately that fish, reptiles, birds, marsupials, and mammals possess red blood, there being, as far as is known, only five principal subclasses of vertebrata.

Our inductions will often be embarrassed by exceptions, real or apparent. We might affirm that all gems are incombustible were not diamond undoubtedly combustible. Nothing seems more evident than that all the metals are opaque until we examine them in fine films, when gold and silver are found to be transparent. All plants absorb carbonic acid except certain fungi; all the bodies of the planetary system have a progressive motion from west to east, except the satellites of Uranus and Neptune. Even some of the profoundest laws of matter are not quite universal; all solids expand by heat except india-rubber, and possibly a few other substances; all liquids which have been tested expand by heat except water below 4°C and fused bismuth; all gases have a coefficient of expansion increasing with the temperature except hydrogen. In a later chapter I shall consider how such anomalous cases may be regarded and classified; here we have only to express them in a consistent manner in our notation.

Let us take the case of the transparency of metals, and assign the terms thus

A = metal
B = gold
C = silver
D = iron
E, F &c. = copper, lead, &c.
X = opaque.

Our premises will be

$$\begin{aligned} A &= B \mathbin{\cdot|\cdot} C \mathbin{\cdot|\cdot} D \mathbin{\cdot|\cdot} E, \text{ \&c.} \\ B &= Bx \\ C &= Cx \\ D &= DX \\ E &= EX, \end{aligned}$$

and so on for the rest of the metals. Now evidently

$$Abc = (D \mathbin{\cdot|\cdot} E \mathbin{\cdot|\cdot} F \mathbin{\cdot|\cdot} \ldots\ldots)bc,$$

and by substitution as before we shall obtain

$$Abc = AbcX,$$

or in words, 'All metals not gold nor silver are opaque;' at the same time we have

$$A(B \mathbin{\cdot|\cdot} C) = AB \mathbin{\cdot|\cdot} AC = ABx \mathbin{\cdot|\cdot} ACx = A(B \mathbin{\cdot|\cdot} C)x,$$

or 'Metals which are either gold or silver are not opaque.'

In some cases the problem of induction assumes a much higher degee of complexity. If we examine the properties of crystallized substances we may find some properties which are common to all, as cleavage or fracture in definite planes; but it would soon become requisite to break up the class into several minor ones. We should divide crystals according to the seven accepted systems—and we should then find that crystals of each system possess many common properties. Thus crystals of the Regular or Cubical system expand equally by heat, conduct heat and electricity with uniform rapidity, and are of like elasticity in all directions; they have but one index of refraction for light; and every facet is repeated in like relation to each of the three axes. Crystals of the system which possess one principal axis will be found to possess the various physical powers of conduction, refraction, elasticity, &c., uniformly in directions perpendicular to the principal axis, but in other directions their properties vary according to complicated laws. The remaining systems in which the crystals possess three unequal axes, or have inclined axes, exhibit still more complicated results, the effects of the crystal upon light, heat, electricity, &c., varying in all directions. But when we pursue induction into the intricacies of its application to Nature we really enter upon the subject of classification which we must take up again in a later part of this work.

Complete Solution of the Inverse or Inductive Logical Problem.

It is now plain that Induction consists in passing back from a series of combinations to the laws by which such combinations are governed. The natural law that all metals are conductors of electricity really means that in nature we find three classes of objects, namely—

1. Metals, conductors;
2. Not-metals, conductors;
3. Not-metals, not-conductors.

It comes to the same thing if we say that it excludes the existence of the class, 'metals not-conductors.' In the same way every other law or group of laws will really mean the exclusion from existence of certain combinations of the things, circumstances or phenomena governed by those laws. Now in logic we treat not the phenomena and laws but, strictly speaking, the general forms of the laws; and a little consideration will show that for a finite number of things the possible number of forms or kinds of law governing them must also be finite. Using general terms we know that A and B can be present or absent in four ways and no more—thus

AB, A*b*, *a*B, *ab*;

therefore every possible law which can exist concerning the relation of A and B must be marked by the exclusion of one or more of the above combinations. The number of possible laws then cannot exceed the number of selections which we can make from these four combinations, and we arrive at this utmost number of cases by omitting any one or more of the four. The number of cases to be considered is therefore 2 × 2 × 2 × 2 or sixteen, since each may be present or absent; and these cases are all shown in the following table, in which the sign 0 indicates absence or non-existence of the combination shown at the left-hand column in the same line, and the mark 1 its presence :—

	1	2	3	4	5	6	7 *	8 *	9	10 *	11	12 *	13	14 *	15 *	16 *
AB	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1
A*b*	0	0	0	0	1	1	1	1	0	0	0	0	1	1	1	1
*a*B	0	0	1	1	0	0	1	1	0	0	1	1	0	0	1	1
ab	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1

Thus in column sixteen we find that all the conceivable combinations are present, which means that there are no special laws in existence in such a case, and that the combinations are governed only by the universal Laws of Identity and Difference. The example of metals and conductors of electricity would be represented by the twelfth column; and every other mode in which two things or qualities might present themselves is shown in one or other of the columns. More than half the cases may indeed be at once rejected, because they involve the entire absence of a term or its negative. It has been shown to be a necessary logical principle that every term must have its negative (p. 88), and where this is not the case some inconsistency between the laws or conditions of combinations must exist. Thus if we laid down the two following propositions, 'Graphite conducts electricity,' and 'Graphite does not conduct electricity,' it would amount to asserting the impossibility of graphite existing at all; or in general terms, A is B and A is not B result in destroying altogether the combinations containing A.

We therefore restrict our attention to those cases which may be represented in natural phenomena where at least two combinations are present, and which correspond to those columns of the table in which each of A, a, B, b appears. These cases are shown in the columns marked with an asterisk.

We find that seven cases remain for examination, thus characterised—

Four cases exhibiting three combinations,
Two cases exhibiting two combinations,
One case exhibiting four combinations.

It has already been pointed out that a proposition of the form $A = AB$ destroys one combination Ab, so that this is the form of law applying to the twelfth case. But by changing one or more of the terms in $A = AB$ into its negative, or by interchanging A and B, a and b, we obtain no less than eight different varieties of the one form; thus—

12th case.	8th case.	15th case.	14th case.
$A = AB$	$A = Ab$	$a = aB$	$a = ab$
$b = ab$	$B = aB$	$b = Ab$	$B = AB$.

But the reader of the preceding sections will at once see that each proposition in the lower line is logically equivalent to, and is in fact the contrapositive of, that above it (p. 98). Thus the propositions $A = Ab$ and $B = aB$ both give the same combinations, shown in the eighth column of the table, and trial shows that the twelfth, eighth, fifteenth and fourteenth cases are thus fully accounted for. We come to this conclusion then—*The general form of proposition* $A = AB$ *admits of four logically distinct varieties, each capable of expression in two different modes.*

In two columns of the table, namely the seventh and tenth, we observe that two combinations are missing. Now a simple identity $A = B$ renders impossible both Ab and aB, accounting for the tenth case; and if we change B into b the identity $A=b$ accounts for the seventh case. There may indeed be two other varieties of the simple identity, namely $a=b$ and $a=B$; but it has already been shown repeatedly that these are equivalent respectively to $A=B$ and $A=b$ (pp. 133, 134). As the sixteenth column has already been accounted for as governed by no special conditions, we come to the following general conclusion:—The laws governing the combinations of two terms must be capable of expression either in a partial identity ($A=AB$), or a simple identity ($A=B$); the partial identity is capable of only four logically distinct varieties, and the simple identity of two. Every logical relation between two terms must be expressed in one of these six laws, or must be logically equivalent to one of them.

In short, we may conclude that in treating of partial and complete identity, we have exhaustively treated the modes in which two terms or classes of objects can be related. Of any two classes it may be said that one must either be included in the other, or must be identical with it, or some similar relation must exist between one class and the negative of the other. We have thus completely solved the inverse logical problem concerning two terms[d].

The Inverse Logical Problem involving Three Terms.

No sooner do we introduce into the problem a third term C, than the investigation assumes a far more complex character, so that some readers may prefer to pass over this section. Three terms and their negatives may be combined, as we have frequently seen, in eight different combinations, and the effect of laws or logical conditions is to destroy any one or more of these combinations. Now we may make selections from eight things in 2^8 or 256 ways; so that we have no less than 256 different cases to treat, and the complete solution is at least fifty times as troublesome as with two terms. Many series of combinations, indeed, are contradictory, as in the simpler problem, and may be passed over. The test of consistency is that each of the letters A, B, C, *a*, *b*, *c* shall appear somewhere in the series of combinations; but I have not been able to discover any mode of calculating the number of cases in which inconsistency would happen. The logical complexity of the problem is so great that the ordinary modes of calculating numbers of combinations in mathematical science fail to give any aid, and exhaustive examination of the combinations in detail is the only method applicable.

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[d] The contents of this and the following section nearly correspond with those of a paper read before the Manchester Literary and Philosophical Society on December 26th, 1871. See Proceedings of the Society, vol. xi. pp. 65–68, and Memoirs, Third Series, vol. v. pp. 119–130.

My mode of solving the problem was as follows:—Having written out the whole of the 256 series of combinations, I examined them separately and struck out such as did not fulfil the test of consistency. I then chose some common form of proposition involving two or three terms, and varied it in every possible manner, both by the circular interchange of letters (A, B, C into B, C, A and then C, A, B), and by the substitution for any one or more of the terms of the corresponding negative terms. For instance, the proposition AB = ABC can be first varied by circular interchange, so as to give BC = BCA and then CA = CAB. Each of these three can then be thrown into eight varieties by negative change. Thus AB = ABC gives *a*B = *a*BC, A*b* = A*b*C, AB = AB*c*, *ab* = *ab*C, and so on. Thus there may possibly exist no less than twenty-four varieties of the law having the general form AB = ABC, meaning that whatever has the properties of A and B has those also of C. It by no means follows that some of the varieties may not be equivalent to others; and trial shows, in fact, that AB = ABC is exactly the same in meaning as Ac = Abc or Bc = Bca. Thus the law in question has but eight varieties of distinct logical meaning. I now ascertain by actual deductive reasoning which of the 256 series of combinations result from each of these distinct laws, and mark them off as soon as found. I now proceed to some other form of law, for instance A = ABC, meaning that whatever has the qualities of A has those also of B and C. I find that it admits of twenty-four variations, all of which are found to be logically distinct; the combinations being worked out, I am able to mark off twenty-four more of the list of 256 series. I proceed in this way to work out the results of every form of law which I can find or invent. If in the course of this work I obtain any series of combinations which had been previously marked off, I learn at once that the law is logically equivalent to some law previously treated. It may be safely inferred that every variety of the apparently new law will coincide in meaning with some variety of the former expression of the same law. I have sufficiently verified this assumption in some cases and have never found it lead to error. Thus just as AB = ABC is equivalent to Ac = Abc, so we find that ab = abC is equivalent to ac = acB.

Among the laws treated were the two A = AB and A = B which involve only two terms, because it may of course happen that among three things two only are in special logical relation, and the third independent; and the series of combinations representing such cases of relation are sure to occur in the complete enumeration. All single propositions which I could invent having been treated, pairs of propositions were next investigated. Thus we have the relations, 'All A's are B's and all B's are C's,' of which the old logical syllogism is the development. We may also have 'all A's are all B's, and all B's are C's,' or even 'all A's are all B's, and all B's are all C's.' All such premises admit of variations, greater or less in number, the logical distinctness of which can only be determined by trial in detail. Disjunctive propositions either singly or in pairs were also treated, but were often found to be equivalent to other propositions of a simpler form; thus $A = ABC + Abc$ is exactly the same in meaning as $AB = AC$.

This mode of exhaustive trial bears some analogy to that ancient mathematical process called the Sieve of Eratosthenes. Having taken a long series of the natural numbers, Eratosthenes is said to have calculated out in succession all the multiples of every number, and to have marked them off, so that at last the prime numbers alone remained, and the factors of every number were exhaustively discovered. My problem of 256 series of combinations is the logical analogue, the chief points of difference being that there is a limit to the number of cases, and that prime numbers have no analogue in logic, since every series of combinations corresponds to a law or group of conditions. But the analogy is perfect in the point that they are both inverse processes. There is no mode of ascertaining that a number is prime but by showing that it is not the product of any assignable factors. So there is no mode of ascertaining what laws are embodied in any series of combinations but trying exhaustively the laws which would give them. Just as the results of Eratosthenes' method have been worked out to a great extent and registered in tables for the convenience of other mathematicians, I have endeavoured to work out the inverse logical problem to the utmost extent which is at present practicable or useful.

I have thus found that there are altogether fifteen conditions or series of conditions which may govern the combinations of three terms, forming the premises of fifteen essentially different kinds of arguments. The following table contains a statement of these conditions, together with the number of combinations which are contradicted or destroyed by each, and the number of logically distinct variations of which the law is capable. There might be also added, as a sixteenth case, that case where no special logical condition exists, so that all the eight combinations remain.

Reference Number.	Propositions expressing the general type of the logical conditions.	Number of distinct logical variations.	Number of combinations contradicted by each.
I.	$A = B$	6	4
II.	$A = AB$	12	2
III.	$A = B$, $B = C$	4	6
IV.	$A = B$, $B = BC$	24	5
V.	$A = AB$, $B = BC$	24	4
VI.	$A = BC$	24	4
VII.	$A = ABC$	24	3
VIII.	$AB = ABC$	8	1
IX.	$A = AB$, $aB = aBc$	24	3
X.	$A = ABC$, $ab = abC$	8	4
XI.	$AB = ABC$, $ab = abc$	4	2
XII.	$AB = AC$	12	2
XIII.	$A = BC + Abc$	8	3
XIV.	$A = BC + bc$	2	4
XV.	$A = ABC$, $a = Bc + bC$	8	5
		192	

There are sixty-three series of combinations derived from self-contradictory premises, which with the above 192 series and the one case where there are no conditions or laws at all, make up the whole conceivable number of 256 series.

We learn from this table, for instance, that two propositions of the form $A = AB$, $B = BC$, which are such as constitute the premises of the old syllogism Barbara, negative or render impossible four of the eight combinations in which three terms may be united, and that these propositions are capable of taking twenty-four variations by transpositions of the terms or the introduction of negatives. This table then presents the results of a complete analysis of all the possible logical relations arising in the case of three terms, and the old syllogism forms but one out of fifteen typical forms. Generally speaking every form can be converted into apparently different propositions; thus the fourth type $A = B$, $B = BC$ may appear in the form $A = ABC$, $a = ab$, or again in the form of three propositions $A = AB$, $B = BC$, $aB = aBc$; but all these sets of premises yield identically the same series of combinations, and are therefore of exactly equivalent logical meaning. The fifth type, or Barbara, can also be thrown into the equivalent forms $A = ABC$, $aB = aBC$ and $A = AC$, $B = A \mathbin{\cdot\!|\!\cdot} aBC$. In other cases I have obtained the very same logical conditions in four modes of statement. As regards mere appearance and mode of statement, the number of possible premises would be almost unlimited.

The most remarkable of all the types of logical condition is the fourteenth, namely $A = BC \mathbin{\cdot\!|\!\cdot} bc$. It is that which expresses the division of a genus into two doubly marked species, and might be illustrated by the example—'Component of the physical universe = matter, gravitating, or not-matter (ether), not-gravitating.'

It is capable of only two distinct logical variations, namely, $A = BC \mathbin{\cdot\!|\!\cdot} bc$ and $A = Bc \mathbin{\cdot\!|\!\cdot} bC$. By transposition or negative change of the letters we can indeed obtain six different expressions of each of these propositions; but when their meanings are analysed, by working out the combinations, they are found to be logically equivalent to one or other of the above two. Thus the proposition $A = BC \mathbin{\cdot\!|\!\cdot} bc$ can be written in any of the following five other modes,

$$a = bC \mathbin{\cdot\!|\!\cdot} Bc, \qquad B = CA \mathbin{\cdot\!|\!\cdot} ca, \qquad b = cA \mathbin{\cdot\!|\!\cdot} Ca,$$
$$C = AB \mathbin{\cdot\!|\!\cdot} ab, \qquad c = aB \mathbin{\cdot\!|\!\cdot} Ab.$$

I do not think it needful at present to publish the complete table of 193 series of combinations and the premises corresponding to each. Such a table enables us by mere inspection to learn the laws obeyed by any set of combinations of three things, and is to logic what a table of factors and prime numbers is to the theory of numbers, or a table of integrals to the higher mathematics. The table already given (p. 161) would enable a person with but little labour to discover the law of any combinations. If there be seven combinations (one contradicted) the law must be of the eighth type, and the proper variety will be apparent. If there be six combinations (two contradicted), either the second, eleventh, or twelfth type applies, and a certain number of trials will disclose the proper type and variety. If there be but two combinations the law must be of the third type, and so on.

The above investigations are complete as regards the possible logical relations of two or three terms. But when we attempt to apply the same kind of method to the relations of four or more terms, the labour becomes impracticably great. Four terms give sixteen combinations compatible with the laws of thought, and the number of possible selections of combinations is no less than 2^{16} or 65,536. The following table shows the extraordinary manner in which the number of possible logical relations increases with the number of terms involved.

Number of terms.	Number of possible combinations.	Number of possible selections of combinations corresponding to consistent or inconsistent logical relations.
2	4	16
3	8	256
4	16	65,536
5	32	4,294,967,296
6	64	18,446,744,073,709,551,616

Some years of continuous labour would be required to ascertain the precise number of types of laws which may govern the combinations of only four things, and but a small part of such laws would be exemplified or capable of practical application in science. The purely logical inverse problem, whereby we pass from combinations to their laws, is solved in the preceding pages, as far as it is likely to be for a long time to come; and it is almost impossible that it should ever be carried more than a single step further.

Distinction between Perfect and Imperfect Induction.

We cannot proceed further with advantage, before noticing the extreme difference which exists between cases of perfect and those of imperfect induction. We call an induction perfect when all the objects or combinations of events which can possibly come under the class treated have been examined. But in the majority of cases it is impossible to collect together, or in any way to investigate, the properties of all portions of a substance or of all the individuals of a race. The number of objects would often be practically infinite, and the greater part of them might be beyond our reach, in the interior of the earth, or in the most distant parts of the Universe. In all such cases induction is said to be imperfect, and affected by more or less uncertainty. As some writers have fallen into much error concerning the functions and relative importance of these two branches of reasoning, I shall have to point out that—

1. Perfect Induction is a process absolutely requisite, both in the performance of imperfect induction and in the treatment of large bodies of facts of which our knowledge is complete.
2. Imperfect Induction is founded on Perfect Induction, but involves another process of inference of a widely different character.

It is certain that if I can draw any inference at all concerning objects not examined, it must be done on the data afforded by the objects which have been examined. If I judge that a distant star obeys the law of gravity, it must be because all other material objects sufficiently known to me obey that law. If I venture to assert that all ruminant animals have cloven hoofs, it is because all ruminant animals which have come to my notice have cloven hoofs. On the other hand I cannot safely say that all cryptogamous plants possess a purely cellular structure, because some such plants have a partially vascular structure. The probability that a new cryptogam will be cellular only can be estimated, if at all, on the ground of the comparative numbers of known cryptogams which are and are not cellular. Thus the first step in every induction will consist in accurately summing up the number of instances of a particular object or phenomenon which have fallen under our observation. Adams and Leverrier, for instance, must have inferred that the undiscovered planet Neptune would obey Bode's law, because *all the planets known at that time obeyed it.* On what principles and on what circumstances the passage from the known to the apparently unknown is warranted, must be carefully discussed in the next section, and in various parts of this work.

It would be a great mistake, however, to suppose that Perfect Induction is in itself useless. Even when the enumeration of objects belonging to any class is complete, and admits of no inference to unexamined objects, the enumeration of our knowledge in a general proposition is a process of so much importance that we may consider it practically necessary. In many cases we may render our investigations exhaustive; all the teeth or bones of an animal; all the cells in a minute vegetable organ; all the caves in a mountain side; all the strata in a geological section; all the coins in a newly found hoard, may be so completely scrutinized that we may make some general assertion concerning them without fear of mistake. Every bone might be proved to consist of phosphate of lime; every cell to enclose a nucleus; every cave to contain remains of extinct animals; every stratum to exhibit signs of marine origin; every coin to be of Roman manufacture. These are cases where our investigation is limited to a definite portion of matter, or a definite area on the earth's surface.

There is another class of cases where induction is naturally and necessarily limited to a definite number of alternatives. Of the regular solids we can say without the least doubt that no one has more than twenty faces, thirty edges, and twenty corners; for by the principles of geometry we learn that there cannot exist more than five regular solids, of each of which we easily observe that the above statements are true. In the theory of numbers, an endless variety of perfect inductions might be made; we can show that no number less than sixty possesses so many divisors, and the like is true of 360[e], for it does not require any very great amount of labour to ascertain and count all the divisors of numbers up to sixty or 360. Similarly I can assert that between 60,041 and 60,077 no prime number occurs, because the exhaustive examination of those who have constructed tables of prime numbers proves it to be so.

In matters of human appointment or history, we can frequently have a complete limitation to the numbers of instances to be included in an induction. We might show that none of the other kings of England reigned so long as George III; that Magna Charta has not been repealed by any subsequent statute; that the propositions of the third book of Euclid treat only of circles; that no part of the works of Galen mentions the fourth figure of the syllogism; that the price of corn in England has never been so high since 1847 as it was in that year; that the price of the English funds has never been lower than it was on the 23rd of January, 1798, when it fell to 47¼.

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[e] Wallis's 'Treatise of Algebra' (1685), p. 22.

It has been urged against this process of Perfect Induction that it gives no new information, and is merely a summing up in a brief form of a multitude of particulars. But mere abbreviation of mental labour is one of the most important aids we can enjoy in the acquisition of knowledge. The powers of the human mind are so limited that multiplicity of detail is alone sufficient to prevent its progress in many directions. Thought would be practically impossible if every separate fact had to be separately thought and treated. Economy of mental power may be considered one of the main conditions on which our elevated intellectual position depends. Most mathematical processes are but abbreviations of the simpler acts of addition and subtraction. The invention of logarithms was one of the most striking additions ever made to human power: yet it was a mere abbreviation of operations which could have been done before had a sufficient amount of labour been available. Similar additions to our power will, it is hoped, be made from time to time, for the number of mathematical problems hitherto solved is but an indefinitely small portion of those which await solution, because the labour they have hitherto demanded renders them impracticable. So it is really throughout all regions of thought. The amount of our knowledge depends upon our powers of bringing it within practicable compass. Unless we arrange and classify facts, and condense them into general truths, they soon surpass our powers of memory, and serve but to confuse. Hence Perfect Induction, even as a process of abbreviation, is absolutely essential to any high degree of mental achievement.

Transition from Perfect to Imperfect Induction.

It is a question of profound difficulty on what grounds we are warranted in inferring the future from the present, or the nature of undiscovered objects from those which we have examined with our senses. We pass from Perfect to Imperfect Induction when once we allow our conclusion to pass, at all events apparently, beyond the data on which it was founded. In making such a step we seem to gain a nett addition to our knowledge; for we learn the nature of what was unknown. We reap where we have never sown. We appear to possess the divine power of creating knowledge, and reaching with our mental arms far beyond the sphere of our own observation. I shall, indeed, have to point out certain methods of reasoning in which we do pass altogether beyond the sphere of the senses, and acquire accurate knowledge which observation could never have given; but it is not imperfect induction that accomplishes such a task. Of imperfect induction itself, I venture to assert that it never makes any real addition to our knowledge, in the meaning of the expression sometimes accepted. As in other cases of inference it merely unfolds the information contained in past observations or events; it merely renders explicit what was implicit in previous experience. It transmutes knowledge, but certainly does not create knowledge.

There is no fact which I shall more constantly keep before the reader's mind in the following pages than that the results of imperfect induction, however well authenticated and verified, are never more than probable. We never can be sure that the future will be as the present. We hang ever upon the Will of the Creator: and it is only so far as He has created two things alike, or maintains the framework of the world unchanged from moment to moment, that our most careful inferences can be fulfilled. All predictions, all inferences which reach beyond their data, are purely hypothetical, and proceed on the assumption that new events will conform to the conditions detected in our observation of past events. No experience of finite duration can be expected to give an exhaustive knowledge of all the forces which are in operation. There is thus a double uncertainty; even supposing the Universe as a whole to proceed unchanged, we do not really know the Universe as a whole. Comparatively speaking we know only a point in its infinite extent, and a moment in its infinite duration. We cannot be sure, then, that our observations have not escaped some fact, which will cause the future to be apparently different from the past; nor can we be sure that the future really will be the outcome of the past. We proceed then in all our inferences to unexamined objects and times on the assumptions—

1. That our past observation gives us a complete knowledge of what exists.
2. That the conditions of things which did exist will continue to be the conditions of things which will exist.

We shall often need to illustrate the character of our knowledge of nature by the simile of a ballot-box, so often employed by mathematical writers in the theory of probability. Nature is to us like an infinite ballot-box, the contents of which are being continually drawn, ball after ball, and exhibited to us. Science is but the careful observation of the succession in which balls of various character usually present themselves; we register the combinations, notice those which seem to be excluded from occurrence, and from the proportional frequency of those which usually appear we infer the probable character of future drawings. But under such circumstances certainty of prediction depends on two conditions:—

1. That we acquire a perfect knowledge of the comparative numbers of balls of each kind within the box.
2. That the contents of the ballot-box remain unchanged.

Of the latter assumption, or rather that concerning the constitution of the world which it illustrates, the logician or physicist can have nothing to say. As the Creation of the Universe is necessarily an act passing all experience and all conception, so any change in that Creation, or, it may be, a termination of it, must likewise be infinitely beyond the bounds of our mental faculties. No science, no reasoning upon the subject, can have any validity; for without experience we are without the basis and materials of knowledge. It is the fundamental postulate accordingly of all inference concerning the future, that there shall be no arbitrary change in the subject of inference; of the probability or improbability of such a change I conceive that our faculties can give no estimate.

The other condition of inductive inference—that we acquire an approximately complete knowledge of the combinations in which events do occur, is at least in some degree within the bounds of our perceptive and mental powers. There are many branches of science in which phenomena seem to be governed by conditions of a most fixed and general character. We have much ground in such cases for believing that the future occurrence of such phenomena may be calculated and predicted. But the whole question now becomes one of probability and improbability. We leave the region of pure logic to enter one in which the number of events is the ground of inference. We do not leave the region of logic; we only leave that where certainty, affirmative or negative, is the result, and the agreement or disagreement of qualities the means of inference. For the future, number and quantity will enter into most of our processes of reasoning; but then I hold that number and quantity are but portions of the great logical domain. I venture to assert that number is wholly logical, both in its fundamental nature and in all its complex developments. Quantity in all its forms is but a development of number. That which is mathematical is not the less logical; if anything it is the more logical, in the sense that it presents logical results in the highest degree of complexity and variety.

Before proceeding then from Perfect to Imperfect Induction I break off in some degree the course of the work, to treat of the logical conditions of number. I shall then employ number to estimate the variety of combinations in which natural phenomena may present themselves, and the probability or improbability of their occurrence under definite circumstances. It is in later parts of the work that I must endeavour to establish, in a complete manner, the notions which I have set forth upon the subject of Imperfect Induction, as applied in the investigation of Nature, which notions may be thus briefly stated:—

1. Imperfect Induction entirely rests upon Perfect Induction for its materials.
2. The logical process by which we seem to pass directly from examined to unexamined cases consists in an inverse and complex application of deductive inference, so that all reasoning may be said to be either directly or inversely deductive.
3. The result is always of a hypothetical character, and is never more than probable.
4. No nett addition is ever made to our knowledge by reasoning; what we know of future events or unexamined objects is only the unfolded contents of our previous knowledge, and it becomes less and less probable as it is more boldly extended to remote cases.

BOOK II.

NUMBER, VARIETY, AND PROBABILITY.

CHAPTER VIII.

PRINCIPLES OF NUMBER.

NOT without much reason did Pythagoras represent the world as ruled by number. Into almost all our acts of clear thought number enters, and in proportion as we can define numerically we enjoy exact and useful knowledge of the Universe. The science of numbers, too, the study of the principles and methods of reasoning in number, has hitherto presented the widest and most practicable training in logic. So free and energetic has been the study of mathematical forms, compared with the forms and laws of logic, that mathematicians have passed far in advance of any pure logicians. Occasionally, in recent times, they have condescended to apply their great algebraic instruments to a reflex advancement of the primary logical science. It is thus that we chiefly owe to profound mathematicians, such as Sir John Herschel, Dr. Whewell, Professor De Morgan or Dr. Boole, the regeneration of logic in the present century, and I entertain no doubt that it is in maintaining a close alliance with the extensive branches of quantitative reasoning that we must look for still further progress in our comprehension of qualitative inference.

I cannot assent, indeed, to the common notion that certainty begins and ends with numerical determination. Nothing is more certain and accurate than logical truth. The laws of identity and difference are the tests of all that is true and certain throughout the range of thought, and mathematical reasoning is cogent only when it conforms to these conditions, of which logic is the first development. And if it be erroneous to suppose that all certainty is mathematical, it is equally an error to imagine that all which is mathematical is certain. Many processes of mathematical reasoning are of most doubtful validity. There are many points of mathematical doctrine which are and must long remain matter of opinion; for instance, the best form of the definition and axiom concerning parallel lines, or the true nature of a limit or a ratio of infinitesimal quantities. In the use of symbolic reasoning questions occur at every point on which the best mathematicians may differ, as Bernouilli and Leibnitz differed irreconcileably concerning the existence of the logarithms of negative quantities[a]. In fact we no sooner leave the simple logical conditions of number, than we find ourselves involved in a mazy and mysterious science of symbols.

Mathematical science enjoys no monopoly, and not even a supremacy in certainty of results. It is the boundless extent and variety of quantitative questions that surprises and delights the mathematical student. When simple logic can give but a bare answer Yes or No, the algebraist raises a score of subtle questions, and brings out a score of curious results. The flower and the fruit, all that is attractive and delightful, fall to the share of the mathematician, who too often despises the pure but necessary stem from which all has arisen. But in no part of human thought can a reasoner cast himself free from the prior conditions of logical correctness. The mathematician is only strong and true as long as he is logical, and if numbers rule the world, it is the laws of logic which rule number.

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[a] Montucla, 'Histoire des Mathématiques,' vol. iii. p. 373.

Nearly all writers have hitherto been strangely content to look upon numerical reasoning as something wholly apart from logical inference. A long divorce has existed between quality and quantity, and it has not been uncommon to treat them as contrasted in nature and restricted to independent branches of human thought. For my own part, I have a profound belief that all the sciences meet somewhere upon common ground. No part of knowledge can stand wholly disconnected from other parts of the great universe of thought; it is incredible, above all, that the two great branches of abstract science, interlacing and co-operating in every discourse, should rest upon totally distinct foundations. I assume that a connection exists, and care only to inquire, What is its nature? Does the science of quantity rest upon that of quality; or, *vice versâ*, does the science of quality rest upon that of quantity? There might conceivably be a third view, that they both rest upon some still deeper set of principles yet undiscovered, but there is an absence of any suggestions to this effect. The late Dr. Boole adopted the second view, and treated logic as a kind of algebra,—a special case of analytical reasoning which admits but the two quantities—unity and zero. He proved beyond doubt that a deep analogy does exist between the forms of algebraic and logical deduction; and could this analogy receive no other explanation we must have accepted his opinion, however strange. But I shall attempt to show that just the reverse explanation is the true one.

I hold that algebra is a highly developed logic, and number but logical discrimination. Logic resembles algebra, as the mould resembles that which is cast in it. Logic has imposed its own laws upon every branch of mathematical science, and it is no wonder that we ever meet with the traces of those laws from the domain of which we can never emerge.

The Nature of Number.

Number is but another name for *diversity.* Exact identity is unity, and with difference arises plurality. An abstract notion, as was pointed out (p. 33), possesses a certain *oneness.* The quality of *justice*, for instance, is one and the same in whatever just acts it be manifested. In justice itself there are no marks of difference by which to discriminate justice from justice. But one just act can be discriminated from another just act by many circumstances of time and place, and we can count and number many acts each thus discriminated from every other. In like manner pure gold is simply pure gold, and is so far one and the same throughout. But besides its intrinsic and invariable qualities, gold occupies space and must have shape or size. Portions of gold are always mutually exclusive and capable of discrimination, at least in respect that they must be each without the other. Hence they may be numbered.

Plurality arises when and only when we detect difference. For instance, in counting a number of gold coins I must count each coin once, and not more than once. Let C denote a coin, and the mark above it the position in the order of counting. Then I must count the coins

$$C' + C'' + C''' + C'''' + \ldots\ldots\ldots$$

If I were to make them as follows

$$C' + C'' + C''' + C''' + C'''' + \ldots\ldots,$$

I should make the third coin into two, and should imply the existence of difference where there is not difference[b]. C''' and C''' are but the names of one coin named twice ever. But according to one of the conditions of logical symbols, which I have called the Law of Unity (p. 86), the same name repeated has no effect, and

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[b] 'Pure Logic,' Appendix, p. 82, § 192.

$$A + A = A.$$

We must apply the Law of Unity, and must reduce all identical alternatives before we can count with certainty and use the processes of numerical calculation. Identical alternatives are harmless in logic, but produce deadly error in number. Thus logical science ascertains the nature of the mathematical unit, and the definition may be given in these terms—*A unit is any object of thought which can be discriminated from every other object treated as a unit in the same problem.*

It has often been said that units are units in respect of being perfectly similar to each other; but though they may be perfectly similar in some respects, they must be different in at least one point, otherwise they would be incapable of plurality. If three coins were so similar that they occupied the same space at the same time, they would not be three coins, but one. It is a property of space that every point is discriminable from every other point, and in time every moment is necessarily distinct from any other moment before or after. Hence we frequently count in space or time, and Locke, with some other philosophers, has even held that number arises from repetition in time. Beats of a pendulum might be so perfectly similar that we could discover no difference except that one beat is before and another after. Time alone is here the ground of difference and is a sufficient foundation for the discrimination of plurality; but it is by no means the only foundation. Three coins are three coins, whether we count them successively or regard them all simultaneously. In many cases neither time nor space is the ground of difference, but pure quality alone enters. We can discriminate for instance the weight, inertia, and hardness of gold as three qualities, though none of these is before or after the other, either in space or time. Every means of discrimination may be a source of plurality.

Our logical notation may be used to express the rise of number. The symbol A stands for one thing or one class, and in itself must be regarded as a unit, because no difference is specified. But the combinations AB and A*b* are necessarily *two*, because they cannot logically coalesce, and there is a mark B which distinguishes one from the other. A logical definition of the number *four* is given in the combinations ABC, AB*c*, A*b*C, A*bc*, where there is a double difference, and as Puck says—

> 'Yet but three? Come one more;
> Two of both kinds makes up four.'

I conceive that all numbers might be represented as arising out of the combinations of the Abecedarium, more or less of each series being struck out by various logical conditions. The number three, for instance, arises from the condition that A must be either B or C, so that the combinations are ABC, AB*c*, A*b*C.

Of Numerical Abstraction.

There will now be little difficulty in forming a clear notion of the nature of numerical abstraction. It consists in abstracting the character of the difference from which plurality arises, retaining merely the fact. When I speak of *three men* I need not at once specify the marks by which each may be known from each. Those marks must exist if they are really three men and not one and the same, and in speaking of them as many I imply the existence of the requisite differences. Abstract number, then, is *the empty form of difference*; the abstract [illegible] which [illegible] different [illegible] in the [illegible] abstract [illegible] gather the [illegible] with the quality that [illegible] which asserts [illegible] together with sufficient [illegible] mark [illegible] such objects [illegible] in the [illegible] supposing that there are marks of [illegible] we use a term in the singular [illegible]

The origin of the great generality of number is now apparent. Three sounds differ from three colours, or three riders from three horses; but they agree in respect of the variety of marks by which they can be discriminated. The symbols 1 + 1 + 1 are thus the empty marks asserting the fact of discrimination which may apply to objects indifferently independently of their peculiar nature.

Concrete and Abstract Numbers.

The common distinction between concrete and abstract numbers can now be easily stated. In proportion as we specify the logical character of the things numbered, we render them concrete. In the abstract number *three* there is no statement of the points in which the three objects agree; but in three coins, three men, or three horses, not only are the variety of objects defined, but their nature is restricted. Concrete number thus implies the same consciousness of difference as abstract number, but it is mingled with a groundwork of similarity expressed in the logical terms. There is similarity or identity so far as logical terms enter; difference so far as the terms are merely numerical.

The reason of the important Law of Homogeneity will now be apparent. This law asserts that in every arithmetical calculation the logical nature of the things numbered must remain unaltered. The specified logical agreement of the things numbered must not be affected by the unspecified numerical differences. A calculation would be palpably absurd which, after commencing with length, gave a result in hours. It is in reality equally absurd in a purely arithmetical point of view to deduce areas from the calculation of lengths, masses from the combination of volume and density, or momenta from mass and velocity. It must remain for subsequent consideration in what sense we may truly say that two linear feet multiplied by two linear feet give four superficial feet, but arithmetically it is absurd, because there is a change of unit.

As a general rule we treat in each calculation only objects of one nature. We do not, and cannot properly add, in the same sum yards of cloth and pounds of sugar. We cannot even conceive the result of adding area to velocity, or length to density, or weight to value. The unit numbered and added must have a basis of homogeneity, or must be reducible to some common denominator. Nevertheless it is quite possible, and in fact common, to treat in one complex calculation the most heterogeneous quantities, on the condition that each kind of object is kept distinct, and treated numerically only in conjunction with its own kind. Different units, so far as their logical differences are specified, must never be substituted one for the other. Chemists continually use equations which assert the equivalence of groups of atoms. Ordinary fermentation is represented by the formula

$$C\ H^{12}O^{6} = 2C^{2}H^{6}O + 2CO^{2}.$$

Three kinds of units, the atoms respectively of Carbon, Hydrogen, and Oxygen, are here intermingled, but there is really a separate equation in regard to each kind. Mathematicians also employ compound equations of the same kind; for in $a + b\sqrt{-1} = c + d\sqrt{-1}$, it is impossible by ordinary addition to add a to $b\sqrt{-1}$. Hence we really have the separate equations $a = c$, and $b = d$[c]. Similarly an equation between two quaternions is equivalent to four equations between ordinary quantities, whence indeed the origin of the name *quaternion.*

Analogy of Logical and Numerical Terms.

If my assertion is correct that number arises out of logical conditions, we ought to find number obeying all the laws and conditions of logic. It is almost superfluous to point out that this is the case with the fundamental laws of identity and difference, and it only remains for me to show that mathematical symbols do really obey the special conditions of logical symbols which were formerly pointed out (p. 39). Thus the Law of Commutativeness, is equally true of quality and quantity. As in logic we have

$$AB = BA,$$

so in mathematics it is familiarly known that

$$2 \times 3 = 3 \times 2, \quad \text{or} \quad x \times y = y \times x.$$

The properties of space, in short, are as indifferent in pure multiplication as we found them in pure logical thought.

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[c] De Morgan's 'Trigonometry and Double Algebra,' p. 126.

Similarly, just as in logic

triangle or square = square or triangle,

or generally $A \cdot|\cdot B = B \cdot|\cdot A$,

so in quantity $2 + 3 = 3 + 2$,

or generally $x + y = y + x$.

The symbol $\cdot|\cdot$ is not identical with $+$, but it is so far analogous.

How far, now, is it true that mathematical symbols obey the law of simplicity expressed in the form

$$AA = A,$$

or the example

Round round = round ?

Apparently there are but two numbers which obey this law; for it is certain that

$$x \times x = x$$

is true only in the two cases when $x = 1$ or 0.

In reality all numbers obey the law, for $2 \times 2 = 2$ is not really analogous to $AA = A$. According to the definition of a unit already given, each unit is discriminated from each other in the same problem, so that in $2' \times 2'$, the first *two* involves a different discrimination from the second *two*. I get four kinds of things, for instance, if I first discriminate 'heavy and light' and then 'cubical and spherical,' for we now have the following classes—

heavy, cubical. light, cubical.
heavy, spherical. light, spherical.

But suppose that my two classes are in both cases discriminated by the same difference of light and heavy, then we have

heavy heavy = heavy,
heavy light = 0,
light heavy = 0,
light light = light.

In short, *twice two is two* unless we take care that the second two has a different meaning from the first. But under similar circumstances logical terms would give exactly the like result, and it is not true that $A'A'' = A'$, identically where A'' is different in meaning from A'.

In an exactly similar manner it may be shown that the Law of Unity

$$A \mathbin{\cdot|\cdot} A = A$$

holds true alike of logical and mathematical terms. It is absurd indeed to say that

$$x + x = x$$

except in the one case when $x =$ absolute zero. But this contradiction $x + x = x$ arises from the fact that we have already defined the unit in one x as differing from those in the other. Under such circumstances the Law of Unity does not apply. For if in

$$A' \mathbin{\cdot|\cdot} A'' = A'$$

we mean that A'' is in any way different from A' the assertion of identity is evidently false.

The contrast then which seems to exist between logical and mathematical symbols is only apparent. It is because the Law of Simplicity and Unity must always be observed in the operation of counting that those laws can no longer be operative. This is the understood condition under which we use all numerical symbols. Whenever I use the symbol 5 I really mean

$$1 + 1 + 1 + 1 + 1,$$

and it is perfectly understood that each of these units is distinct from each other. If requisite I might mark them thus

$$1' + 1'' + 1''' + 1'''' + 1'''''.$$

Were this not the case and were the units really

$$1' + 1'' + 1'' + 1''' + 1'''',$$

the Law of Unity would, as before remarked, apply, and

$$1'' + 1'' = 1''.$$

Mathematical symbols then obey all the laws of logical symbols, but two of these laws seem to be inapplicable simply because they are presupposed in the definition of the mathematical unit. Logic thus lays down the conditions of number, and the science of arithmetic developed as it is into all the wondrous branches of mathematical calculus is but an outgrowth of logical discrimination.

Principle of Mathematical Inference.

As I have asserted, the universal principle of all reasoning is that which allows us to substitute like for like. I have now to point out that in the mathematical sciences this principle is involved in each step of reasoning. It is in these sciences indeed that we meet with the clearest cases of substitution, and it is the simplicity with which the principle can be applied which probably led to the comparatively early perfection of the sciences of geometry and arithmetic. Euclid, and the Greek mathematicians from the first, recognised *equality* as the fundamental relation of quantitative thought, but Aristotle rejected the exactly analogous, but far more general relation of identity, and thus crippled the formal science of logic as it has descended to the present day.

Geometrical reasoning starts from the Axiom that 'things equal to the same thing are equal to each other.' Two equalities enable us to infer a third equality; and this is true not only of lines and angles, but of areas, volumes, numbers, intervals of time, forces, velocities, degrees of intensity, or, in short, anything which is capable of being equal or unequal. Two stars equally bright with the same star must be equally bright with each other, and two forces equally intense with a third force are equally intense with each other. It is remarkable that Euclid has not expressly stated two other axioms, the truth of which is necessarily implied. The second axiom should be that 'Two things of which one is equal and the other unequal to a third common thing, are unequal to each other.' An equality and inequality, in short, may give an inequality, and this is equally true with the first axiom of all kinds of quantity. If Venus, for instance, agrees with Mars in density, but Mars differs from Jupiter, then Venus differs from Jupiter. A third axiom must exist to the effect that 'Things unequal to the same thing may or may not be equal to each other.' *Two inequalities give no ground of inference whatever.* If we only know, for instance, that Mercury and Jupiter differ in density from Mars, we cannot say whether or not they agree between themselves. As a fact they do not agree; but Venus and Mars on the other hand both differ from Jupiter and yet closely agree with each other. The force of the axioms can be most clearly illustrated by drawing lines[d].

The general conclusion must be then that where there is equality there may be inference, but where there is not equality there cannot be inference. A plain induction will lead us to believe that *equality is the condition of inference concerning quantity.* All the three axioms may in fact be summed up in one, to the effect, that '*in whatever relation one quantity stands to another, it stands in the same relation to the equal of that other.*'

The active power is always the substitution of equals, and it is an accident that in a pair of equalities we can make the substitution in two ways. From $a = b = c$ we can infer $a = c$, either by substituting in $a = b$ the value of b as given in $b = c$, or else by substituting in $b = c$ the value of b as given in $a = b$. In $a = b \backsim d$ we can make but the one substitution of a for b. In $e \backsim f \backsim g$ we can make no substitution and get no inference.

In mathematics the relations in which terms may stand to each other are far more varied than in pure logic, yet our principle of substitution always holds true. We may say in the most general manner that *In whatever relation one quantity stands to another, it stands in the same relation to the equal of that other.* In this axiom we sum up a number of axioms which have been stated in more or less detail by algebraists[e]. Thus, 'If equal quantities be added to equal quantities, the sums will be equal.' To explain this, let

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[d] 'Elementary Lessons in Logic' (Macmillan), p. 123.

$$a = b, \qquad c = d.$$

Now $a + c$, whatever it means, must be identical with itself, so that

$$a + c = a + c.$$

In one side of this equation substitute for the quantities their equivalents, and we have the axiom proved

$$a + c = b + d.$$

The similar axiom concerning subtraction is equally evident, for whatever $a - c$ may mean it is equal to $a - c$, and therefore by substitution to $b - d$. Again, 'if equal quantities be multiplied by the same or equal quantities, the products will be equal.' For evidently

$$ac = ac,$$

and if for c in one side we substitute its equal d, we have

$$ac = ad,$$

and a second similar substitution gives us

$$ac = bd.$$

We might prove a like axiom concerning division in an exactly similar manner. I might even extend the list of axioms and say that 'Equal powers of equal number are equal.' For certainly, whatever $a \times a \times a$ may mean, it is equal to $a \times a \times a$; hence by our usual substitution

$$a \times a \times a = b \times b \times b,$$

or

$$a^3 = b^3.$$

The truth will hold of roots, that is to say,

$$\sqrt[c]{a} = \sqrt[d]{b},$$

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[e] Todhunter's 'Algebra,' 3rd ed. p. 40.

provided that the same roots are taken, that is that the root of a shall really be related to a as the root of b is to b. The ambiguity of meaning of an operation thus fails in any way to shake the universality of the principle.

We may go further and assert that, not only the above common relations, but all other known or conceivable mathematical relations obey the same principle. Let Pa denote in the most general manner that we do something with the quantity a; then if $a=b$ it follows that

$$\mathrm{P}a = \mathrm{P}b.$$

Let us make Pa, for instance, mean

$$a^3 - 3a^2 + 2a + 5;$$

then it necessarily follows that this quantity is exactly equal to

$$b^3 - 3b^2 + 2b + 5.$$

The reader will also remember that one of the most frequent operations in mathematical reasoning is to substitute for a quantity its equal, as known either by assumed, natural, or self-evident condition. Whenever a quantity appears twice over in a problem, we may apply what we learn of its relations in one place to its relations in the other. All reasoning in mathematics, as in other branches of science, thus involves the principle of treating equals equally, or similars similarly. In whatever way we employ quantitative reasoning in the remaining parts of this work, we never can desert the simple principle on which we first set out.

Reasoning by Inequalities.

I have stated that all the processes of mathematical reasoning may be deduced from the principle of substitution. Exceptions to this assertion may seem to exist in the use of inequalities. The greater of a greater is undoubtedly a greater, and what is less than a less is certainly less. Snowdon is higher than the Wrekin, and Ben Nevis than

Snowdon; therefore Ben Nevis is higher than the Wrekin. But a little consideration discloses much reason for believing that even in such cases, where equality does not apparently enter, the force of the reasoning entirely depends upon underlying and implied equalities.

In the first place, two statements of mere difference do not give any ground of inference. We learn nothing concerning the comparative heights of St. Paul's and Westminster Abbey from the assertions that they both differ in height from St. Peter's at Rome. Thus we need something more than mere inequality; we require one identity in addition, namely the identity in direction of the two differences. Thus we cannot employ inequalities in the simple way in which we do equalities, and, when we try to express exactly what other conditions are requisite, we shall find ourselves lapsing into the use of equalities or identities.

In the second place, every argument by inequalities may be represented with at least equal clearness and force in the form of equalities. Thus we clearly express that a is greater than b by the equation

$$a = b + p, \qquad (1)$$

where p is an intrinsically positive quantity, denoting the difference of a and b. Similarly we express that b is greater than c by the equation

$$b = c + q, \qquad (2)$$

and substituting for b in (1) its value in (2) we have

$$a = c + q + p. \qquad (3)$$

Now as p and q are both positive, it follows that a is greater than c, and we have the exact amount of excess specified. It will be easily seen that the reasoning concerning that which is less than a less will result in an equation of the form

$$c = a - q - p.$$

Every argument by inequalities may then be thrown into the form of an equality; but the converse is not true. We cannot possibly prove that two quantities are equal by merely asserting that they are both greater or both less than another quantity. From $e > f$ and $g > f$, or $e < f$ and $g < f$, we can infer no relation between e and g. And if the reader take the equations $x = y = 3$ and attempt to prove that therefore $x = 3$, by throwing them into inequalities, he will find it impossible to do so.

From these considerations I gather that reasoning in arithmetic or algebra by so-called inequalities is only an imperfectly expressed reasoning by equalities, and when we want to exhibit exactly and clearly the conditions of reasoning, we are obliged to use equalities explicitly. Just as in pure logic a negative proposition, as expressing mere difference, cannot be the means of inference, so inequality can never really be the true ground of inference. I do not deny that affirmation and negation, agreement and difference, equality and inequality, are pairs of equally fundamental relations, but I assert that inference is possible only where affirmation, agreement, or equality, some species of identity in fact, is present, explicitly or implicitly.

Arithmetical Reasoning.

It might seem somewhat inconsistent that I assert number to arise out of difference or discrimination, and yet hold that no reasoning can be grounded on difference. Number, of course, opens a most wide sphere for inference, and a little consideration shows that this is due to the unlimited series of identities which spring up out of numerical abstraction. If six people are sitting on six chairs, there is no resemblance between the chairs and the people in logical character. But if we overlook all the qualities both of a chair and a person, and merely remember that there are marks by which each of six chairs may be discriminated from each other, and similarly with the people, then there arises a resemblance between the chairs and people, and this resemblance in number may be the ground of inference. If on another occasion the chairs are filled by people again, we may infer that these people must resemble the others in number, though they need not resemble them in any other points.

Groups of units are what we really treat in arithmetic. The number *five* is really $1 + 1 + 1 + 1 + 1$, but for the sake of conciseness we substitute the more compact sign 5, or the name *five*. These names being arbitrarily imposed in any one manner, an indefinite variety of relations spring up between them which are not in the least arbitrary. If we define *four* as $1 + 1 + 1 + 1$, and *five* as $1 + 1 + 1 + 1 + 1$, then of course it follows that *five* = *four* + 1; but it would be equally possible to take this latter equality as a definition, in which case one of the former equalities would become an inference. It is hardly requisite to decide how we define the names of numbers, provided we remember that out of the infinitely numerous relations of one number to others, some one relation expressed in an equality must be a definition of the number in question and the other relations immediately become necessary inferences.

In the science of number the variety of classes which can be formed is altogether infinite, and statements of perfect generality may be made subject only to difficulty or exception at the lower end of the scale. Every existing number for instance belongs to the class

$$m + 7;$$

that is, every number must be the sum of another number and seven, except of course the first six or seven numbers, negative quantities not being here taken into account. Every number is the half of some other, and so on. The subject of generalization, as exhibited in arithmetical or mathematical truths, is an indefinitely wide one. In number we are only at the first step of an extensive series of generalizations. A number is general as compared with the particular things numbered, so we may have general symbols for numbers, or general symbols not for numbers, but for the relations between undetermined numbers. There is, in fact, an unlimited hierarchy of successive generalizations.

Numerically Definite Reasoning.

It was first discovered by Prof. de Morgan that many arguments are valid which combine logical and numerical reasoning, although they could in no way be included in the ancient logical formulas. He developed the doctrine of the 'Numerically Definite Syllogism,' fully explained in his 'Formal Logic' (pp. 141–170). Dr. Boole also devoted considerable attention to the determination of what he called 'Statistical Conditions,' meaning the numerical conditions of logical classes. In a paper published among the Memoirs of the Manchester Literary and Philosophical Society, Third Series, vol. IV. p. 330 (Session 1869–70), I have pointed out that we can apply arithmetical calculation to the Logical Abecedarium. Having given certain logical conditions and the numbers of objects in certain classes, we can either determine the number of objects in other classes governed by those conditions, or can show what further data are required to determine them. As an example of the kind of questions treated in numerical logic, and the mode of treatment, I give the following problem suggested by De Morgan, with my mode of representing its solution[f].

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[f] It has been pointed out to me by Mr. A. J. Ellis, F.R.S., that my solution, as given in the Memoirs of the Manchester Philosophical Society, does not exactly answer to the conditions of the problem, and I therefore substitute above a more satisfactory solution.

'For every man in the house there is a person who is aged; some of the men are not aged. It follows that some persons in the house are not men[g].'

Now let A = person in house,
B = male,
C = aged.

By enclosing logical symbols in brackets, let us denote the number of objects belonging to the class indicated by the symbol. Thus let

(A) = number of persons in house,
(AB) = number of male persons in house,
(ABC) = number of aged male persons in house,

and so on. Now if we use w and w' to denote unknown and indefinite numbers, the conditions of the problem may be thus stated according to my interpretation of the words—

$$(\mathrm{AB}) = (\mathrm{AC}) - w, \qquad (1)$$

that is to say, the number of persons in the house who are aged is at least equal to, and may exceed, the number of male persons in the house;

$$(\mathrm{AB}c) = w', \qquad (2)$$

that is to say, the number of male persons in the house who are not aged is some unknown positive quantity.

If we develop the terms in (1) by the Law of Duality (pp. 87, 95, 97), we obtain

$$(\mathrm{ABC}) + (\mathrm{AB}c) = (\mathrm{ABC}) + (\mathrm{A}b\mathrm{C}) - w.$$

Subtracting the common term (ABC) from each side and substituting for (AB*c*) its value as given in (2), we get at once

$$(\mathrm{A}b\mathrm{C}) = w + w',$$

and adding (A*bc*) to each side, we have

$$(\mathrm{A}b) = \mathrm{A}bc + w + w'.$$

The meaning of this result is that the number of persons in the house who are not men is at least equal to $w + w'$, and exceeds it by the number of persons in the house who are neither men nor aged (A*bc*).

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[g] 'Syllabus of a proposed System of Logic,' p. 29.

It should be understood that this solution applies only to the terms of the example quoted above, and not to the general problem for which De Morgan intended it to serve as an illustration.

As a second instance, let us take the following question:—The whole number of voters in a borough is a; the number against whom objections have been lodged by liberals is b; and the number against whom objections have been lodged by conservatives is c; required the number, if any, who have been objected to on both sides. Taking

A = voter,
B = objected to by liberals,
C = objected to by conservatives,

then we require the value of (ABC). Now the following equation in identically true—

$$(ABC) = (AB) + (AC) + (Abc) - (A). \quad (1)$$

For if we develop all the terms on the second side we obtain

$$(ABC) = (ABC) + (ABc) + (ABC) + (AbC) + (Abc) - (ABC) - (ABc) - (AbC) - (Abc);$$

and striking out the corresponding positive and negative terms, we have only left (ABC) = (ABC). Since then (1) is necessarily true, we have only to insert the known values, and we have

$$(ABC) = b + c - a + (Abc).$$

Hence the number who have received objections from both sides is equal to the excess, if any, of the whole number of objections over the number of voters together with the numbers of voters who have received no objections (A*bc*).

In many cases classes of objects may exist under special logical conditions, and we must consider how these conditions must be interpreted numerically.

Every logical proposition or equation now gives rise to a corresponding numerical equation. Sameness of qualities occasions sameness of numbers. Hence if

$$A = B$$

denotes the identity of the qualities of A and B, we may conclude that

$$(A) = (B).$$

It is evident that exactly those objects, and those objects only, which are comprehended under A must be comprehended under B. It follows that wherever we can draw an equation of qualities, we can draw a similar equation of numbers. Thus, from

$$A = B = C$$

we infer

$$A = C;$$

and similarly from

$$(A) = (B) = (C),$$

meaning the numbers of A's and C's are equal to the number of B's, we can infer

$$(A) = (C).$$

But, curiously enough, this does not apply to negative propositions and inequalities. For if

$$A = B \sim D$$

means that A is identical with B, which differs from D, it does not follow that

$$(A) = (B) \sim (D).$$

Two classes of objects may differ in qualities, and yet they may agree in number. This is a point which strongly confirms me in the opinion I have already expressed, that all inference really depends upon equations, not differences (p. 186).

The Logical Abecedarium thus enables us to make a complete analysis of any numerical problem, and though the symbolical statement may sometimes seem prolix, I conceive that it really represents the course which the mind must follow in solving the question. Although thought may seem to outstrip the rapidity with which the symbols can be written down, yet the mind does not really follow a different course from that indicated by the symbols. For a fuller explanation of this natural system of Numerically Definite Reasoning, with more abundant illustrations and an analysis of De Morgan's Numerically Definite Syllogism, I must refer the reader to the paper in the Memoirs of the Manchester Literary and Philosophical Society, as already referred to, portions of which, however, have been embodied in the present section.

The reader may be referred, also, to Boole's writings upon the subject in the 'Laws of Thought,' chap. xix. p. 295, and in a paper on 'Propositions Numerically Definite,' communicated by De Morgan, in 1868, to the Cambridge Philosophical Society, and printed in their 'Transactions,' vol. xi. part ii. Mr. Alexander J. Ellis treats the same subject in his 'Contributions to Formal Logic,' read to the Royal Society, in March, 1872, but as yet published only in the form of a brief abstract, in the Proceedings of the Society, vol. xx. p. 307.

CHAPTER IX.

THE VARIETY OF NATURE, OR THE DOCTRINE OF COMBINATIONS AND PERMUTATIONS.

Nature may be said to be evolved from the monotony of non-existence by the creation of diversity. It is plausibly asserted that we are conscious only so far as we experience difference. Life is change, and perfectly uniform existence would be no better than non-existence. Certain it is that life demands incessant novelty, and that nature though it probably never fails to obey the same fixed laws, yet presents to us an apparently unlimited series of varied combinations of events. It is the work of science to observe and record the kinds and comparative numbers of such combinations of phenomena, occurring spontaneously or produced by our interference. Patient and skilful examination of the records may then disclose the laws imposed on matter at its creation, and enable us more or less successfully to predict, or even to regulate, the future occurrence of any particular combination.

The Laws of Thought are the first and most important of all the laws which govern the combinations of phenomena; and, even though they be binding on the mind, they may also be regarded as verified in the external world. The Logical Abecedarium develops the utmost variety of things and events which may occur, and it is evident that as each new quality is introduced, the number of combinations is doubled. Thus four qualities may occur in 16 combinations; five qualities in 32; six qualities in 64; and so on. In general language, if n be the number of qualities, 2^n is the number of varieties of things which may be formed from them, if there be no conditions but those of logic. This number, it need hardly be said, increases after the first few terms, in an extraordinary manner, so that it would require 302 figures, even to express the number of combinations in which 1000 qualities might conceivably present themselves.

If all the combinations allowed by the Laws of Thought occurred in nature, then science would begin and end with those laws. To observe nature would give us no additional knowledge, because no two qualities would in the long run be oftener associated than any other two. We could never predict events with more certainty than we now predict the throws of dice, and experience would be without use. But the universe, as actually created, presents a far different and much more interesting problem. The most superficial observation shows that some things are habitually associated with other things. The more mature our examination, the more we become convinced that each event depends upon the prior occurrence of some other series of events. Action and reaction are gradually discovered to underlie the whole scene, and an independent or casual occurrence does not exist except in appearance. Even dice as they fall are surely determined in their course by prior conditions and fixed laws. Thus the combinations of events which can really occur are found to be very restricted, and it is the work of science to detect these restricting conditions.

In the English alphabet, for instance, we have twenty-six letters. Were the combinations of such letters perfectly free, so that any letter could be indifferently sounded with any other, the number of words which could be formed without any repetition would be $2^{26} - 1$, or 67,108,863, equal in number to the combinations of the twenty-seventh column of the Abecedarium, excluding one for the case in which all the letters would be absent. But the formation of our vocal organs prevents our using the far greater part of these conjunctions of letters. At least one vowel must be present in each word; more than two consonants cannot usually be brought together; and to produce words capable of smooth utterance a number of other rules must be observed. To determine exactly how many words might exist in the English language under these circumstances, would be an exceedingly complex problem, the solution of which has never been attempted. The number of existing English words may perhaps be said not to exceed one hundred thousand, and it is only by investigating the combinations presented in the dictionary, that we can learn the Laws of Euphony or calculate the possible number. In this example we have an epitome of the work and method of science. The combinations of natural phenomena are limited by a great number of conditions which are in no way brought to our knowledge except so far as they are disclosed in the examination of nature.

It is often a very difficult matter to determine the numbers of permutations or combinations which may exist under various restrictions. Many learned men puzzled themselves in former centuries over what were called Protean verses, or Latin verses admitting many variations in accordance with the Laws of Metre. The most celebrated of these verses was that invented by Bernard Bauhusius, as follows[a]:—

'Tot tibi sunt dotes, Virgo, quot sidera cœlo.'

One author, Ericius Puteanus, filled forty-eight pages of a work in reckoning up its possible transpositions, making them only 1022. Other calculators gave 2196, 3276, 2580 as their results. Dr. Wallis assigned 3096, but without much confidence in the accuracy of his result.[b] It required the skill of James Bernouilli to decide the number of transpositions to be 3312, under the condition that the sense and metre of the verse shall be perfectly preserved.

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[a] Montucla, 'Histoire,' &c., vol. iii. p. 388.

In approaching the consideration of the great Inductive problem, it is very necessary that we should acquire correct notions as to the comparative number of combinations which may exist under different circumstances. The doctrine of combinations is that part of mathematical science which applies numerical calculation to determine the number of combinations under various conditions. It is a part of the science which really lies at the base not only of other sciences, but of other branches of mathematical science. The forms of algebraical expressions are determined by the principles of combination, and Hindenburg recognised this fact in his Combinatorial Analysis. The greatest mathematicians have, during the last three centuries, given their best powers to the treatment of this subject; it was the favourite study of Pascal; it early attracted the attention of Leibnitz, who wrote his curious essay, *De Arte Combinatoria*, at twenty years of age; James Bernouilli, one of the very profoundest mathematicians, devoted no small part of his life to the investigation of the subject as connected with that of Probability; and in his celebrated work, *De Arte Conjectandi*, he has so finely described the importance of the doctrine of combinations, that I need offer no excuses for quoting his remarks at full length. 'It is easy to perceive that the prodigious variety which appears both in the works of nature and in the actions of men, and which constitutes the greatest part of the beauty of the universe, is owing to the multitude of different ways in which its several parts are mixed with, or placed near, each other. But, because the number of causes that concur in producing a given event, or effect, is oftentimes so immensely great, and the causes themselves are so different one from another, that it is extremely difficult to reckon up all the different ways in which they may be arranged or combined together, it often happens that men, even of the best understandings and greatest circumspection, are guilty of that fault in reasoning which the writers on logic call *the insufficient or imperfect enumeration of parts or cases:* insomuch that I will venture to assert, that this is the chief, and almost the only, source of the vast number of erroneous opinions, and those too very often in matters of great importance, which we are apt to form on all the subjects we reflect upon, whether they relate to the knowledge of nature or the merits and motives of human actions. It must therefore be acknowledged, that that art which affords a cure to this weakness, or defect, of our understandings, and teaches us so to enumerate all the possible ways in which a given number of things may be mixed and combined together, that we may be certain that we have not omitted any one arrangement of them that can lead to the object of our inquiry, deserves to be considered as most eminently useful and worthy of our highest esteem and attention. And this is the business of *the art or doctrine of combinations.* Nor is this art or doctrine to be considered merely as a branch of the mathematical sciences. For it has a relation to almost every species of useful knowledge that the mind of man can be employed upon. It proceeds indeed upon mathematical principles, in calculating the number of the combinations of the things proposed: but by the conclusions that are obtained by it, the sagacity of the natural philosopher, the exactness of the historian, the skill and judgment of the physician, and the prudence and foresight of the politician may be assisted; because the business of all these important professions is but *to form reasonable conjectures* concerning the several objects which engage their attention, and all wise conjectures are the results of a just and careful examination of the several different effects that may possibly arise from the causes that are capable of producing them.' [c]

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[b] Wallis, 'Of Combinations,' &c., p. 119.

Distinction of Combinations and Permutations.

We must at once consider the deep difference which exists between Combinations and Permutations; a difference involving important logical principles, and influencing the form of all our mathematical expressions. In *permutation* we recognise varieties of order or arrangement, treating AB as a different group from BA. In *combination* we take notice only of the presence or absence of a certain thing, and pay no regard to its place in order of time or space. Thus the four letters *a, e, m, n* can form but one combination, but they occur in language in several permutations, as *name, amen, mean, mane.*

We have hitherto been dealing with purely logical questions, involving only combination of qualities. I have fully pointed out in more than one place that, though our symbols could not but be written in order of place and read in order of time, the relations expressed had no regard to place or time (pp. 40, 131). The Law of Commutativeness, in fact, expresses the condition that in logic we deal with combinations, and the same law is true of all the processes of algebra. In nature and art, order may be a matter of indifference; it makes no difference, for instance, whether gunpowder is a mixture of sulphur, carbon and nitre, or carbon, nitre and sulphur, or nitre, sulphur and carbon, provided that the substances are present in proper proportions and well mixed. But this indifference of order does not usually extend to the events of physical science or the operations of art. The change of mechanical energy into heat is not exactly the same as the change from heat into mechanical energy; thunder does not indifferently precede and follow lightning; it is a matter of some importance that we load, cap, present, and fire a rifle in this precise order. Time is the condition of all our thoughts, space of all our actions, and therefore both in art and science we are to a great extent concerned with permutations. All language, for instance, treats different permutations of letters as having different meanings.

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[c] James Bernouilli, 'De Arte Conjectandi,' translated by Baron Maseres. London, 1795, pp. 35–36.

Permutations of certain things are far more numerous than combinations of those things, for the obvious reason that each distinct thing is regarded differently according to its place. Thus the letters A, B, C, will make different permutations according as A stands first, second, or third; having decided the place of A, there are two places between which we may choose for B; and then there remains but one place for C. Accordingly the permutations of these letters will be altogether $3 \times 2 \times 1$ or 6 in number. With four things or letters, A, B, C, D, we shall have four choices of place for the first letter, three for the second, two for the third, and one for the fourth, so that there will be altogether $4 \times 3 \times 2 \times 1$, or 24 permutations. The same simple rule applies in all cases; beginning with the whole number of things we multiply at each step by a number decreased by a unit. In general language, if n be the number of things in a combination, the number of permutations is $n(n-1)(n-2) \dots . 4 . 3 . 2 . 1$. Thus, if we were to re-arrange the names of the days of the week, the possible arrangements out of which we should have to choose the new order, would be no less than $7 . 6 . 5 . 4 . 3 . 2 . 1$, or 5040, or, excluding the existing order, 5039.

The reader will see that the numbers which we reach in questions of permutation, increase in a more extraordinary manner even than in combination. Each new object or term doubles the number of combinations (p. 195), but increases the permutations by a factor continually growing. Instead of $2 \times 2 \times 2 \times 2 \times \ldots\ldots$ we have $2 \times 3 \times 4 \times 5 \times \ldots\ldots$ and the products of the latter expression indefinitely exceed those of the former. These products of continually increasing factors are constantly employed, as we shall see, in questions both of permutation and combination. They are technically called *factorials*, that is to say, the product of all integer numbers, from unity up to any number n, is the *factorial* of n, and is often indicated symbolically by $\underline{|n}$. I give below the factorials up to that of fifteen :—

$$\begin{aligned}
6 &= 1 \,.\, 2 \,.\, 3 \\
24 &= 1 \,.\, 2 \,.\, 3 \,.\, 4 \\
120 &= 1 \,.\, 2 \ldots\ldots 5 \\
720 &= 1 \,.\, 2 \ldots\ldots 6 \\
5{,}040 &= \underline{|7} \\
40{,}320 &= \underline{|8} \\
362{,}880 &= \underline{|9} \\
3{,}628{,}800 &= \underline{|10} \\
39{,}916{,}800 &= \underline{|11} \\
479{,}001{,}600 &= \underline{|12} \\
6{,}227{,}020{,}800 &= \underline{|13} \\
87{,}178{,}291{,}200 &= \underline{|14} \\
1{,}307{,}674{,}368{,}000 &= \underline{|15}
\end{aligned}$$

The factorials up to $\underline{|36}$ are given in Rees' 'Cyclopædia,' art. *Cipher*, and the logarithms of products up to $\underline{|265}$ are given at the end of the table of logarithms published under the superintendence of the Society for the Diffusion of Useful Knowledge (p. 215). To express the factorial $\underline{|265}$ would require 529 places of figures.

Many writers have from time to time remarked upon the extraordinary magnitude of the numbers with which we deal in this subject. Tacquet calculated[d] that the twenty-four letters of the alphabet may be arranged in more than 620 thousand trillions of orders; and Schottus estimated[e] that if a thousand millions of men were employed for the same number of years in writing out these arrangements, and each man filled each day forty pages with forty arrangements in each, they could not have accomplished the task, as they would have written only 584 thousand trillions instead of 620 thousand trillions.

In some questions the number of permutations may be restricted and reduced by various conditions. Some things in a group may be undistinguishable from others, so that change of order will produce no difference. Thus if we were to permutate the letters of the name *Ann*, according to our previous rule, we should obtain $3 \times 2 \times 1$, or 6 orders; but half of these arrangements would be identical with the other half, because the interchange of the two *n*'s has no effect. The really different orders will therefore be $\frac{3 \cdot 2 \cdot 1}{1 \cdot 2}$ or 3, namely *Ann*, *Nan*, *Nna*. In the word *utility* there are two *i*'s and two *t*'s, in respect of both of which pairs the number of permutations must be halved. Thus we obtain $\frac{7 \cdot 6 \cdot 5 \cdot 4 \cdot 3 \cdot 2 \cdot 1}{1 \cdot 2 \cdot 1 \cdot 2}$ or 1260, as the number of permutations. The simple rule evidently is that when some things or letters are undistinguished, proceed in the first place to calculate all the possible permutations as if all were different, and then divide by the number of possible permutations of those series of things which are not distinguished, and of which the permutations have therefore been counted in excess. Thus since the word *Utilitarianism* contains fourteen letters, of which four are *i*'s, two *a*'s, and two *t*'s, the number of distinct arrangements will be found by dividing the factorial of 14, by the factorials of 4, 2, and 2, the result being 908,107,200. From the letters of the word *Mississippi* we can get in like manner $\frac{|\underline{11}}{|\underline{4} \times |\underline{4} \times |\underline{2}}$ or 34,650 permutations, or not one-thousandth part of what we should obtain were all the letters different.

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[d] 'Arithmeticæ Theoria.' Ed. Amsterd. 1704, p. 517.

[e] Rees' 'Cyclopædia,' art. *Cipher*.

Calculation of Number of Combinations.

Although in many questions both of art and science we need to calculate the number of permutations on account of their own interest, it far more frequently happens in scientific subjects that they possess but an indirect interest. As I have already pointed out, we almost always deal in the logical and mathematical sciences with *combinations*, and variety of order enters only through the inherent imperfections of our symbols and modes of calculation. Signs must be used in some order, and we must withdraw our attention from this order before the signs correctly represent the relations of things which exist neither before nor after each other. Now, it often happens that we cannot choose all the combinations of things, without first choosing them subject to the accidental variety of order, and we must then divide by the number of possible variations of order, that we may get to the true number of pure combinations.

Suppose that we wish to determine the number of ways in which we can select three letters out of the alphabet, without allowing the same letter to be repeated. At the first choice we can take any one of 26 letters; at the next step there remain 25 letters, any one of which may be joined with that already taken; at the third step there will be 24 choices, so that apparently the whole number of ways of choosing is $26 \times 25 \times 24$. But the fact that one choice succeeded another has caused us to obtain the same combinations of letters in different orders; we should get, for instance, a, p, r at one time, and p, r, a at another, and every three distinct letters will appear six times over, because three things can be arranged in six permutations. Thus the true number of combinations will be $\frac{24 \times 23 \times 22}{1 \times 2 \times 3}$, or 2024.

It is apparent that we need the doctrine of permutations in order that we may in many questions counteract the exaggerating effect of successive selection. If out of a senate of 30 persons we have to choose a committee of 5, we may choose any of 30 first, any of 29 next, and so on, in fact there will be $30 \times 29 \times 28 \times 27 \times 26$ selections; but as the actual character of the members of the committee will not be affected by the accidental order of their selection, we divide by $1 \times 2 \times 3 \times 4 \times 5$, and the possible number of different committees will be 142,506. Similarly if we want to calculate the number of ways in which the eight major planets may come into conjunction, it is evident that they may meet either two at a time or three at a time, or four or more at a time, and as nothing is said as to the relative order or place in the conjunction, we require the number of combinations. Now a selection of 2 out of 8 is possible in $\frac{8.7}{1.2}$ or 28 ways; of 3 out of 8 in $\frac{8.7.6}{1.2.3}$ or 56 ways; of 4 out of 8 in $\frac{8.7.6.5}{1.2.3.4}$ or 70 ways; and it may be similarly shown that for 5, 6, 7, and 8 planets, meeting at one time, the number of ways is 56, 28, 8 and 1. Thus we have solved the whole question of the variety of conjunctions of eight planets; and adding all the numbers together, we find that 247 is the utmost possible number of modes of meeting.

In general algebraic language, we may say that a group of m things may be chosen out of a total number of n things, in a number of combinations denoted by the formula

$$\frac{n.(n-1)\ (n-2)\ (n-3)\ \ldots.\ (n-m+1)}{1\ .\ 2\ .\ 3\ .\ 4\ \ldots.\ m}$$

The extreme importance and significance of this formula seems to have been first adequately recognised by Pascal, although its discovery is attributed by him to a friend, M. de Ganières.[f] We shall find it perpetually recurring in questions both of combinations and probability, and throughout the formulæ of mathematical analysis traces of its influence will be noticed.

The Arithmetical Triangle.

The Arithmetical Triangle is a name long since given to a series of remarkable numbers connected with the subject we are treating. According to Montucla[g] 'this triangle is in the theory of combinations and changes of order, almost what the table of Pythagoras is in ordinary arithmetic, that is to say, it places at once under the eyes, the numbers required in a multitude of cases of this theory.' As early as 1544 Stifels had noticed the remarkable properties of these numbers and the mode of their evolution.[h] Briggs, the inventor of the common system of logarithms, was so struck with their importance that he called them the Abacus Panchrestus. Pascal, however, was the first who wrote a distinct treatise on these numbers, and gave them the name by which they are still known. But Pascal did not by any means exhaust the subject, and it remained for James Bernouilli to demonstrate fully the importance of the *figurate numbers*, as they are also called. In his treatise *De Arte Conjectandi*, he points out their application in the theory of combinations and probabilities, and remarks of the Arithmetical Triangle, 'It not only contains the clue to the mysterious doctrine of combinations, but it is also the ground or foundation of most of the important and abstruse discoveries that have been made in the other branches of the mathematics.'[1]

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[f] 'Œuvres Complètes de Pascal' (1865), vol. iii. p. 302. Montucla states the name as De Gruières, 'Histoire des Mathématiques,' vol. iii. p. 389.

[g] 'Histoire des Mathématiques,' vol. iii. p. 387.

[h] Leslie, 'Dissertation on the Progress of Mathematical and Physical Science,' Encyclopædia Britannica.

The numbers of the triangle can be calculated in a very easy manner by successive additions. We commence with unity at the apex; in the next line we place a second unit to the right of this; to obtain the third line of figures we move the previous line one place to the right, and add them to the same figures as they were before removal, and we can then repeat the same process *ad infinitum*. The fourth line of figures, for instance, contains 1, 3, 3, 1; moving them one place and adding as directed we obtain :—

Fourth line . . .	1	3	3	1			
		1	3	3	1		
Fifth line	1	4	6	4	1		
		1	4	6	4	1	
Sixth line	1	5	10	10	5	1	
		1	5	10	10	5	1
Seventh line . . .	1	6	15	20	15	6	1

Carrying out this simple process through ten more steps we obtain the first seventeen lines of the Arithmetical Triangle as printed on the next page. Theoretically speaking the Triangle must be regarded as infinite in extent but the numbers increase so rapidly that it soon becomes almost impracticable to continue the table. The longest table of the numbers which I have found is given in Fortia's 'Traité des Progressions' (p. 80), where they are given up to the fortieth line and the ninth column.

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[1] Bernouilli, 'De Arte Conjectandi,' translated by Francis Maseres, London, 1795, p. 75.

THE ARITHMETICAL TRIANGLE.

Line.	First Column.	Second Column.	Third Column.	Fourth Column.	Fifth Column.	Sixth Column.	Seventh Column.	Eighth Column.	Ninth Column.	Tenth Column.	Eleventh Column.	Twelfth Column.	Thirteenth Column.	Fourteenth Column.	Fifteenth Column.	Sixteenth Column.	Seventeenth Col.
1	1																
2	1	1															
3	1	2	1														
4	1	3	3	1													
5	1	4	6	4	1												
6	1	5	10	10	5	1											
7	1	6	15	20	15	6	1										
8	1	7	21	35	35	21	7	1									
9	1	8	28	56	70	56	28	8	1								
10	1	9	36	84	126	126	84	36	9	1							
11	1	10	45	120	210	252	210	120	45	10	1						
12	1	11	55	165	330	462	462	330	165	55	11	1					
13	1	12	66	220	495	792	924	792	495	220	66	12	1				
14	1	13	78	286	715	1287	1716	1716	1287	715	286	78	13	1			
15	1	14	91	364	1001	2002	3003	3432	3003	2002	1001	364	91	14	1		
16	1	15	105	455	1365	3003	5005	6435	6435	5005	3003	1365	455	105	15	1	
17	1	16	120	560	1820	4368	8008	11440	12870	11440	8008	4368	1820	560	120	16	1

contain the *trianguli-pyramidal*, the *pyramidi-pyramidal* numbers, and so on.[k]

3. From the mode of formation of the table, it follows that the differences of the numbers in each column will be found in the preceding column to the left. Hence the *second differences*, or the *differences of differences* will be in the second column to the left of any given column, the third differences in the third column, and so on. Thus we may say that unity which appears in the first column is the *first difference* of the numbers in the second column; the *second difference* of those in the third column; the *third difference* of those in the fourth, and so on. The triangle is thus seen to be a complete classification of all numbers according as they have unity for any of their differences.

4. Every number in the table is equal to the sum of the numbers which stand higher in the next column to the left, beginning with the next line above; thus 84 is equal to the sum of 28, 21, 15, 10, 6, 3, 1.

5. Since each line is formed by adding the previous line to itself, it is evident that the sum of the numbers in each horizontal line must be double that of the line next above. Hence we know, without making any additions, that the successive sums must be 1, 2, 4, 8, 16, 32, 64, &c., the same as the numbers of combinations in the Logical Abecedarium. Speaking generally, the sum of the numbers in the nth line will be 2^{n-1}.

6. If the whole of the numbers down to any line be added together, we shall obtain a number less by unity than some power of 2; thus, the first line gives 1 or 2^1-1; the first two lines give 3 or 2^2-1; the first three lines 7 or 2^3-1; the first six lines give 63 or 2^6-1; or, speaking in general language, the sum of the first n lines is 2^n-1.

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[k] Wallis's 'Algebra,' Discourse of Combinations, &c. p. 109.

the number of ways in which I can select 1, 2, 3, 4, 5, 6, 7, and 8 things, out of 8 things. In general language, if I wish to know in how many ways *m* things can be selected in combinations out of *n* things, I must look in the $n + 1^{\text{th}}$ line, and take the $m + 1^{\text{th}}$ number, counting from the left, as the answer. In how many ways, for instance, can a sub-committee of five be chosen out of a committee of nine. The answer is 126, and is the sixth number in the tenth line; it will be found equal to $\frac{9 \cdot 8 \cdot 7 \cdot 6 \cdot 5}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5}$, which our previous formula (p. 206) would give.

The full utility of the figurate numbers will be more apparent when we reach the subject of probabilities, but I may give an illustration or two in this place. In how many ways can we arrange four pennies as regards head and tail? The question amounts to asking in how many ways we can select 0, 1, 2, 3, or 4 heads out of 4 heads, and the *fifth* line of the triangle gives us the complete answer, thus—

We can select No head and 4 tails in 1 way.
„ 1 head and 3 tails in 4 ways.
„ 2 heads and 2 tails in 6 ways.
„ 3 heads and 1 tail in 4 ways.
„ 4 heads and 0 tail in 1 way.

The total number of different cases is 16, or 2^4, and when we come to the next chapter, it will be found that these numbers give us the respective probabilities of all throws with four pennies.

I gave in p. 205 a calculation of the number of ways in which eight planets can meet in conjunction; the reader will find all the numbers detailed in the ninth line of the arithmetical triangle. The sum of the whole line is 2^8 or 256; but we must subtract a unit for the case where no planet appears, and 8 for the 8 cases in which only one in which we can calculate the whole number of combinations of certain things. Either we may take the whole number at once as shown in the Abecedarium, in which case the number will be some power of two, or else we may calculate successively, by aid of permutations, the number of combinations of none, one, two, three, and so on. Hence we arrive at a necessary identity between two series of numbers. In the case of four things we shall have

$$2^4 = 1 + \frac{4}{1} + \frac{4 \cdot 3}{1 \cdot 2} + \frac{4 \cdot 3 \cdot 2}{1 \cdot 2 \cdot 3} + \frac{4 \cdot 3 \cdot 2 \cdot 1}{1 \cdot 2 \cdot 3 \cdot 4}.$$

In a general form of expression we shall have

$$2^n = 1 + \frac{n}{1} + \frac{n \cdot (n-1)}{1 \cdot 2} + \frac{n\,(n-1)\,(n-2)}{1 \cdot 2 \cdot 3} + \&c.,$$

the terms being continued until they cease to have any value. Thus we have arrived at a proof of simple cases of the Binomial Theorem, of which each column of the Abecedarium is an exemplification. It may be shown that all other mathematical expansions likewise arise out of simple processes of combination, but the more complete consideration of this subject must be deferred.

Possible Variety of Nature and Art.

We cannot adequately understand the difficulties which beset us in certain branches of science, unless we gain a clear idea of the vast number of combinations or permutations which may be possible under certain conditions. Thus only can we learn how hopeless it would be to attempt to treat nature in detail, and exhaust the whole number of events which might arise. It is instructive to consider, in the first place, how immensely great are the numbers of combinations with which we deal in many arts and amusements.

into account the semitones, it would become apparent that it is practically impossible to exhaust the variety of music.

Similar considerations apply to the possible number of natural substances, though we cannot always give precisely numerical results. It was recommended by Hatchett[q] that a systematic examination of all alloys of metals should be carried out, proceeding from the most simple binary ones to more complicated ternary or quaternary ones. He can hardly have been aware of the extent of his proposed inquiry. If we operated only upon thirty of the known metals, the number of possible selections of binary alloys would be 435, of ternary alloys 4060, of quaternary 27,405, without paying any regard to the varying proportions of the metals, and only regarding the kind of metal. If we varied all the ternary alloys by quantities not less than one per cent., the number of these alloys only would be 11,445,060. An exhaustive investigation of the subject is therefore out of the question, and unless some laws connecting the properties of the alloy and its components can be discovered, it is not apparent how our knowledge of them can be ever more than most incomplete.

The possible variety of definite chemical compounds, again, is enormously great. Chemists have already examined many thousands of inorganic substances, and a still greater number of organic compounds;[r] they have nevertheless made no appreciable impression on the number which may exist. Taking the number of elements at sixty-one, the number of compounds containing different selections of four elements each would be more than half a million (521,855). As the same elements often combine in many different proportions, and some of them, especially carbon, have the power of forming an almost endless number of compounds, it would hardly be possible to assign any limit to the number of chemical compounds which may be formed. There are branches of physical science, therefore, of which it is unlikely that scientific men, with all their industry, can ever obtain a knowledge in any appreciable degree approaching to completeness.

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[q] 'Philosophical Transactions' (1803), vol. xciii. p. 193.

[r] Hofmann's 'Introduction to Chemistry,' p. 36.

Higher Orders of Variety.

The consideration of the facts already given in this chapter will not produce an adequate notion of the possible variety of existence, unless we consider the comparative numbers of combinations of different orders. By a combination of a higher order, I mean a combination of groups, which are themselves combinations of simpler groups. The almost unlimited number of compounds of carbon, hydrogen, and oxygen, described in organic chemistry, are combinations of a second order, for the atoms are groups of groups. The wave of sound produced by a musical instrument may be regarded as a combination of motions; the body of sound proceeding from a large orchestra is therefore a complex aggregate of sounds each in itself a complex combination of movements. All literature may be said to be developed out of the difference of white paper and black ink. From the almost unlimited number of marks which might be chosen we select twenty-six customary letters. The pronounceable combinations of letters are probably some trillions in number. Now, as a sentence is a certain selection of words, the possible sentences must be indefinitely more numerous than the words of which it may be composed. A book is a combination of sentences, and a library is a combination of books. A library, therefore, is a combination of the fifth order, and the powers of numerical expression would be almost exhausted in attempting to express the number of distinct libraries which might be constructed. The calculation would not be possible, because the union of letters in words, of words in sentences, and of sentences in books, are governed by conditions so complex as to defy calculation. I wish only to point out that there is no limit to the multitude of different sentences which may be developed out of the one difference of ink and paper. Galileo is said to have remarked that all truth is contained in the compass of the alphabet. We might add that it is all contained in the difference of ink and paper.

One consequence of this power of successive combination is that the simplest signals or marks will suffice to express any information. Francis Bacon proposed for secret writing a biliteral cipher, which resolves all letters of the alphabet into permutations of the two letters *a* and *b*. Thus A was *aaaaa*, B *aaaab*, X *babab*, and so on.[s] And in a similar way, as Bacon clearly saw, any one difference can be made the ground of a code of signals; we can express, as he says, *omnia per omnia*. The Morse alphabet uses only a succession of long and short marks, and other systems of telegraphic language employ right and left strokes. A single lamp obscured at various intervals, long or short, may be made to spell out any words, and with two lamps, distinguished by colour or position, we could at once represent Bacon's biliteral alphabet. Mr. Babbage ingeniously suggested that every lighthouse in the world should be made to spell out its own name or number perpetually, by flashes or obscurations of figures required in writing it down, without using about 19,729 figures for the purpose.

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[s] 'Works,' edited by Shaw, vol. i. pp. 141—145, quoted in Rees' 'Encyclopædia,' art. *Cipher*.

The successive orders of the powers of two have then the following values :—

First order	2
Second order	4
Third order	16
Fourth order	65,536
Fifth order, number expressed by	19,729 figures.
Sixth order, number expressed by figures, to express the number of which figures would require about	19,729 figures.

It may give us a powerful notion of infinity to remember that at this sixth step, having long surpassed all bounds of conception, we have made no approach to the goal. Nay, were we to make a hundred such steps, we should be as far away as ever from actual infinity.

It is well worth observing that our powers of expression rapidly overcome the possible multitude of finite objects which may exist in any assignable space. Archimedes showed long ago, in one of the most wonderful writings of antiquity,[t] that the grains of sand in the world could be numbered, or rather, that if numbered, the result could readily be expressed in arithmetical notation. Let us extend his problem, and ascertain whether we could express the number of atoms which could exist in the visible universe. The most distant stars which can now be seen by telescopes —those of the sixteenth magnitude—are supposed to have a distance of about 33,900,000,000,000,000 miles.[u] Sir W. Thomson, again, has shown reasons for supposing

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[t] 'Liber de Arenæ Numero.'

[u] Chambers's 'Astronomy' (1861), p. 272.

Does probability exist in the things which are probable, or in the mind which regards them as such? The etymology of the name lends us no assistance: for, curiously enough, *probable* is ultimately the same word as *provable*, a good instance of one word becoming differentiated to two opposite meanings.

Chance cannot be the subject of the theory, because there is really no such thing as chance,[a] regarded as producing and governing events. This name signifies *falling*, and the notion is continually used as a simile to express uncertainty, because we can seldom predict how a die, or a coin, or a leaf will fall, or when a bullet will hit the mark. But every one knows, on a little reflection, that it is in our knowledge the deficiency lies, not in the certainty of nature's laws. There is no doubt in lightning as to the point it shall strike; in the greatest storm there is nothing capricious; not a grain of sand lies upon the beach, but infinite knowledge would account for its lying there; and the course of every falling leaf is guided by the same principles of mechanics as rule the motions of the heavenly bodies.

Chance then exists not in nature, and cannot co-exist with knowledge; it is merely an expression for our ignorance of the causes in action, and our consequent inability to predict the result, or to bring it about infallibly. In nature the happening of a physical event has been pre-determined from the first fashioning of the universe. Probability belongs wholly to the mind; this indeed is proved by the fact that different minds may regard the very same event at the same time with totally different degrees of probability. A steam-vessel, for instance, is *missing* and some persons believe that she has sunk in mid-ocean; others think differently. In the event itself there can be no such uncertainty; the steam-vessel either has sunk or has not sunk, and no subsequent discussion of the probable nature of the event can alter the fact. Yet the probability of the event will really vary from day to day, and from mind to mind, according as the slightest information is gained regarding the vessels met at sea, the weather prevailing there, the signs of wreck picked up, or the previous condition of the vessel. Probability thus belongs to our mental condition, to the light in which we regard events, the occurrence or non-occurrence of which is certain in themselves. Many writers accordingly have asserted that probability is concerned with degree or quantity of belief. De Morgan says,[b] 'By degree of probability we really mean or ought to mean degree of belief.' The late Professor Donkin expressed the meaning of probability as 'quantity of belief;' but I have never felt satisfied with such a definition of probability. The nature of *belief* is not more clear to my mind than the notion it is used to define. But an all-sufficient objection is, that *the theory does not measure what the belief is, but what it ought to be.* Few minds think in close accordance with the theory, and there are many cases of evidence in which the belief existing is habitually different from what it ought to be. Even if the state of belief in any mind could be measured and expressed in figures, the results would be worthless. The very value of the theory consists in correcting and guiding our belief, and rendering our states of mind and consequent actions harmonious with our knowledge of exterior conditions.

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[a] *Dufau.* 'De la Méthode d'Observation,' chap. iii.

This objection has been clearly perceived by some of those who still used quantity of belief as a definition of probability. Thus De Morgan adds—'Belief is but another name for imperfect knowledge.' Professor Donkin has

[b] 'Formal Logic,' p. 172.

well said that the quantity of belief is 'always relative to a particular state of knowledge or ignorance; but it must be observed that it is absolute in the sense of not being relative to any individual mind; since, the same information being presupposed, all minds *ought* to distribute their belief in the same way.'[c] Dr. Boole, too, seemed to entertain a like view, when he described the theory as engaged with 'the equal distribution of ignorance,'[d] but we may just as well say that it is engaged with the equal distribution of knowledge.

I prefer to dispense altogether with this obscure word belief, and to say that the theory of probability deals with *quantity of knowledge*, an expression of which a precise explanation and measure can presently be given. An event is only probable when our knowledge of it is diluted with ignorance, and exact calculation is needed to discriminate how much we do and do not know. The theory has been described by some as professing *to evolve knowledge out of ignorance;* but as Professor Donkin has admirably remarked, it is really 'a method of avoiding the erection of belief upon ignorance.'[e] It defines rational expectation by measuring the comparative amounts of knowledge and ignorance, and teaches us to regulate our action with regard to future events in a way which will, in the long run, lead to the least amount of disappointment and injury. It is, as Laplace as happily expressed it, *good sense reduced to calculation.*

This theory appears to me the noblest creation of human intellect, and it passes my conception how two men possessing such high intelligence as Auguste Comte and J. S. Mill, could have been found depreciating it, or even vainly attempting to question its validity. To there is a margin of error which can only be safely treated by the principles of probability.

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[c] 'Philosophical Magazine,' 4th Series, vol. i. p. 355.

[d] 'Transactions of the Royal Society of Edinburgh,' vol. xxi. part iv.

[e] 'Philosophical Magazine,' 4th Series, vol i. p. 355.

The method which we employ in the theory consists in calculating the number of all the cases or events concerning which our knowledge is equal. If we have even the slightest reason for suspecting that one event is more likely to occur than another, we should take this knowledge into account. This being done, we must determine the whole number of events which are, so far as we know, equally likely. Thus, if we have no reason for supposing that a penny will fall more often one way than another, there are two cases, head and tail, equally likely. But if from trial or otherwise we know, or think we know, that of 100 throws 55 will give tail, then the probability is measured by the ratio of 55 to 100.

The mathematical formulæ of the theory are exactly the same as those of the theory of combinations. In this latter theory, we determine in how many ways events may be joined together, and we now proceed to use this knowledge in calculating the number of ways in which a certain event may come about, and thus defining its probability. If we throw three pennies into the air, what is the probability that two of them will fall tail uppermost? This amounts to asking in how many possible ways can we select two tails out of three, compared with the whole number of ways in which the coins can be placed. Now, the fourth line of the Arithmetical Triangle (p. 208) gives us the answer. The whole number of ways in which we can select or leave three things is eight, and the possible combinations of two things at a time is three; hence the probability of two tails is the ratio of three to eight. From the numbers in the triangle we may draw all the following probabilities:—

One combination gives 0 tail. Probability $\frac{1}{8}$.
Three combinations give 1 tail. Probability $\frac{3}{8}$.

probabilities that A is B, $\frac{1}{2}$ and that B is C, $\frac{1}{2}$, we have no right to suppose that the probability of A being C is reduced by the argument in its favour. If the conclusion is true on its own grounds, the failure of the argument does not affect it; thus its total probability is its antecedent probability, added to the probability that this failing, the new argument in question establishes it. There is a probability $\frac{1}{2}$ that we shall not require the special argument; a probability $\frac{1}{2}$ that we shall, and a probability $\frac{1}{4}$ that the argument does in that case establish it. Thus the complete result is $\frac{1}{2} + \frac{1}{2} \times \frac{1}{4}$, or $\frac{5}{8}$. In general language, if a be the probability formed on a particular argument, and c the antecedent probability, then the general result is

$$1 - (1 - a)(1 - c), \text{ or } a + c - ac.$$

We may put it still more generally in this way:—Let a, b, c, d, &c., be the probabilities of a conclusion grounded on various arguments or considerations of any kind. It is only when all the arguments fail that our conclusion proves finally untrue; the probabilities of each failing are respectively $1 - a$, $1 - b$, $1 - c$, &c.; the probability that they will all fail $(1 - a)(1 - b)(1 - c)\ldots$; therefore the probability that the conclusion will not fail is $1 - (1 - a)(1 - b)(1 - c)\ldots$&c. On this principle it follows that every argument in favour of a fact, however flimsy and slight, adds probability to it. When it is unknown whether an overdue vessel has foundered or not, every slight indication of a lost vessel will add some probability to the belief of its loss, and the disproof of any particular evidence will not disprove the event.

We must apply these principles of evidence with great care, and observe that in a great proportion of cases the adducing of a weak argument does tend to the disproof of its conclusion. The assertion may have in itself great inherent improbability as being opposed to other evidence reasoning, yet a diversity arises in their application. Similarity of conditions between the events from which we argue, and those to which we argue, must always be the ground of inference; but this similarity may have regard either to time or place, or the simple logical combination of events, or to any conceivable junction of circumstances involving quality, time, and place. Having met with many pieces of substance possessing ductility, and a bright yellow colour, and having discovered, by perfect induction, that they all possess in addition a high specific gravity, and a freedom from the corrosive action of acids, we are led to expect that every piece of substance, possessing like ductility, and a similar yellow colour, will have an equally high specific gravity, and a like freedom from corrosion by acids. This is a case of the co-existence of qualities; for the character of the specimens examined alters not with time or place.

In a second class of cases, time will enter as a principal ground of similarity. When we hear a clock pendulum beat moment after moment, at equal intervals, and with a uniform sound, we confidently expect that the stroke will continue to be repeated uniformly. A comet having appeared several times at nearly equal intervals, we infer that it will probably appear again at the end of another like interval. A man who has returned home evening after evening for many years, and found his house standing, may, on like grounds, expect that it will be standing the next evening, and on many succeeding evenings. Even the continuous existence of an object in an unaltered state, or the finding again of that which we have hidden, is but a matter of inference to be decided by experience.

A still larger and more complex class of cases involves the relations of space, in addition to those of time and quality. Having observed that every triangle drawn upon the diameter of a circle, with its apex upon the circumference, apparently contains a right angle, we may ascertain that all triangles in similar circumstances will contain right angles. This is a case of pure space reasoning, apart from circumstances of time or quality, and it seems to be governed by different principles of reasoning. I shall endeavour to show, however, that geometrical reasoning differs but in degree from that which applies to other natural relations. If we observe that the components of a binary star have moved for a length of time in elliptic curves, we have reason to believe that they will continue so to move. Time and space relations are here complicated together.

The Relation of Cause and Effect.

In a very large part of the scientific investigations which must be considered, we deal with events which follow from previous events, or with existences which succeed existences. Science, indeed, might arise even were material nature a fixed and changeless whole. Endow mind with the power to travel about, and compare part with part, and it could certainly draw inferences concerning the similarity of forms, the co-existence of qualities, or the preponderance of a particular kind of matter in a changeless world. A solid universe, in at least approximate equilibrium, is not inconceivable, and then the relation of cause and effect would evidently be no more than the relation of before and after. As nature exists, however, it is a progressive existence, ever moving and changing as time, the great independent variable, proceeds. Hence it arises that we must continually compare what is happening now with what happened a moment before, and a moment before that moment, and so on, until we reach indefinite periods of past time. A comet is seen moving in the sky, or its constituent particles illumine the heavens with their tails of fire. We cannot explain the present movements of such a body without supposing its prior existence, with a determinate amount of energy and direction of motion; nor can we validly suppose that our task is concluded when we find that it came wandering to our solar system through the unmeasured vastness of surrounding space. Every event must have a cause, and that cause again a cause, until we are lost in the obscurity of the past, and are driven to the belief in one First Cause, by whom the whole course of nature was determined.

Fallacious Use of the Term Cause.

The words Cause and Causation have given rise to infinite trouble and obscurity, and have in no slight degree retarded the progress of science. From the time of Aristotle, the work of philosophy has been often described as the discovery of the causes of things, and Francis Bacon adopted the notion when he said[a] '*vere scire esse per causas scire.*' Even now it is not uncommonly supposed that the knowledge of causes is something different from other knowledge, and consists, as it were, in getting possession of the keys of nature. A single word may thus act as a spell, and throw the clearest intellect into confusion, as I have often thought that Locke was thrown into confusion when endeavouring to find a meaning for the word *power*.[b] In Mr. Mill's 'System of Logic' the term *cause* seems to have reasserted its old noxious power. Not only does Mr. Mill treat the Laws of Causation as almost co-extensive with

[a] 'Novum Organum,' bk. ii. Aphorism 2.

[b] 'Essay on the Human Understanding,' bk. ii. chap. xxi.

science, but he so uses the expression as to imply that when once we pass within the circle of causation we deal with certainties.

The philosophical danger which attaches to the use of this word may be thus described. A cause is defined as the necessary or invariable antecedent of an event, so that when the cause exists the effect will also exist or soon follow. If then we know the cause of an event, we know when it will certainly happen; and as it is implied that science, by a proper experimental method, may attain to a knowledge of causes, it follows that experience may give us a certain knowledge of future events. Now, nothing is more unquestionable than that finite experience can never give us certain knowledge of the future, so that either a cause is not an invariable antecedent, or else we can never gain certain knowledge as to causes. The first horn of this dilemma is hardly to be accepted. Doubtless there is in nature some invariably acting mechanism, such that from certain fixed conditions an invariable result always emerges. But we, with our finite minds and short experience, can never penetrate the mystery of those existences which embody the Will of the Creator, and evolve it throughout time. We are in the position of spectators who witness the productions of a complicated machine, but are not allowed to examine its intimate structure. We learn what does happen and what does appear, but if we ask for the reason, the answer would involve an infinite depth of mystery. The simplest bit of matter, or the most trivial incident, such as the stroke of two billiard balls, offers infinitely more to learn than ever the human intellect can fathom. The word cause covers just as much untold meaning as any of the words *substance, matter, thought, existence.*

Confusion of Two Questions.

The subject is much complicated, too, by the confusion of two distinct questions. An event having happened, we may ask—

(1) Is there any cause for the event?

(2) Of what kind is that cause?

No one would assert that the mind possesses any faculty capable of inferring, prior to experience, that the occurrence of a sudden noise with flame and smoke indicates the combustion of a black powder, formed by the mixture of black, white, and yellow powders. The greatest upholder of *à priori* doctrines will allow that the particular aspect, shape, size, colour, texture, and other qualities of a cause must be gathered from experience and through the senses.

The question whether there is any cause at all for an event, is of a totally different kind. If an explosion could happen without any prior existing conditions, it must be a new creation—a distinct addition to the universe. It may be plausibly held that we can imagine neither the creation nor annihilation of anything. As regards matter, this has long been held true; as regards force, it is now almost universally assumed as an axiom that energy can neither come into nor go out of existence without distinct acts of Creative Will. That there exists any instinctive belief to this effect, indeed, seems doubtful. We find Lucretius, a philosopher of the utmost intellectual power and cultivation, gravely assuming that his raining atoms could turn aside from their straight paths in a self-determining manner, and by this spontaneous origination of energy determine the form of the universe.[c] Sir George Airy, too, seriously discussed the mathematical conditions under which a perpetual motion, that is, a perpetual source of self-created energy might exist.[d] The larger part of the philosophic world has long held that in mental acts there is free will—in short, self-causation. It is in vain to attempt to reconcile this doctrine with that of an intuitive belief in causation, as Sir W. Hamilton candidly allowed.

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[c] 'De Rerum Natura,' bk. ii. ll. 216–293.

It is quite obvious, moreover, that to assert the existence of a cause for every event, cannot do more than remove into the indefinite past the inconceivable fact and mystery of creation. At any given moment matter and energy were equal to what they are at present, or they were not; if equal, we may make the same inquiry concerning any other moment, however long prior, and we are thus obliged to accept one horn of the dilemma—existence from infinity, or creation at some moment. This is but one of the many cases in which we are compelled to believe in one or other of two alternatives, both inconceivable. My present purpose, however, is to point out that we must not confuse this supremely difficult question with that into which inductive science inquires on the foundation of facts. By induction we gain no certain knowledge; but by observation, and the inverse use of deductive reasoning, we estimate the probability that an event which has occurred was preceded by conditions of specified character, or that such conditions will be followed by the event.

Definition of the Term Cause.

Clear definitions of the word cause have been given by several philosophers. Hobbes has said, 'A cause is the sum or aggregate of all such accidents both in the agents and the patients, as concur in the producing of the effect propounded; all which existing together, it cannot be understood but that the effect existeth with them; or that it can possibly exist if any of them be absent.' Dr. Brown, in his 'Essay on Causation,' gave a nearly corresponding statement. 'A cause,' he says[e], 'may be defined to be the object or event which immediately precedes any change, and which existing again in similar circumstances will be always immediately followed by a similar change.' Of the kindred word *power*, he likewise says:[f] 'Power is nothing more than that invariableness of antecedence which is implied in the belief of causation.'

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[d] 'Cambridge Philosophical Transactions,' [1830] vol. iii. pp. 369–372.

These definitions may be accepted with the qualification that our knowledge of causes in such a sense can be probable only. The work of science consists in ascertaining the combinations in which phenomena present themselves. Concerning every event we shall have to determine its probable conditions, or group of antecedents from which it probably follows. An antecedent is anything which exists prior to an event; a consequent is anything which exists subsequently to an antecedent. It will not usually happen that there is any probable connection between an antecedent and consequent. Thus nitrogen is an antecedent to the lighting of a common fire; but it is so far from being a cause of the lighting, that it renders the combustion less active. Daylight is an antecedent to all fires lighted during the day, but it probably has no appreciable effect one way or the other. But in the case of any given event it is usually possible to discover a certain number of antecedents which seem to be always present, and with more or less probability we conclude that when they exist the event will follow.

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[e] 'Observations on the Nature and Tendency of the Doctrine of Mr. Hume, concerning the Relation of Cause and Effect.' Second ed. p. 44.

[f] Ibid. p. 97.

Let it be observed that the utmost latitude is at present enjoyed in the use of the term *cause.* Not only may a cause be an existent thing endowed with powers, as oxygen is the cause of combustion, gunpowder the cause of explosion, but the very absence or removal of a thing may also be a cause. It is quite correct to speak of the dryness of the Egyptian atmosphere, or the absence of moisture, as being the cause of the preservation of mummies, and other remains of antiquity. The cause of a mountain elevation, Ingleborough for instance, is the excavation of the surrounding valleys by denudation. It is not so usual to speak of the existence of a thing at one moment as the cause of its existence at the next, but to me it seems the commonest case of causation which can occur. The cause of motion of a billiard ball may be the stroke of another ball; and recent philosophy leads us to look upon all motions and changes, as but so many manifestations of prior existing energy. In all probability there is no creation of energy and no destruction, so that as regards both mechanical and molecular changes, the cause is really the manifestation of existing energy. In the same way I see not why the prior existence of matter is not also a cause as regards its subsequent existence. All science tends to show us that the existence of the universe in a particular state at one moment, is the condition of its existence at the next moment, in an apparently different state. When we analyse the meaning which we can attribute to the word *cause*, it amounts to the existence of suitable portions of matter endowed with suitable quantities of energy. If we may accept Horne Tooke's assertion, *cause* has etymologically the meaning of *thing before.* Though, indeed, the origin of the word is very obscure, its derivatives the Italian *cosa,* and the French *chose,* mean simply *thing.* In the German equivalent *ursache,* we have plainly the original meaning of *thing before,* the *sache* denoting 'interesting or important object,' the English *sake,* and *ur* being the equivalent of the English *ere, before*[h]. We abandon, then, both etymology and philosophy, when we attribute to the *laws of causation* any meaning beyond that of the *conditions* in which an event may be expected to happen, according to our observation of the previous course of nature.

I have no objection to use the words cause and causation, provided they are never allowed to lead us to imagine that our knowledge of nature can attain to certainty. I repeat that if a cause is an invariable and necessary condition of an event, we can never know certainly whether the cause exists or not. To us, then, a cause is not to be distinguished from the group of positive or negative conditions which, with more or less probability, precede an event. In this sense, there is no particular difference between knowledge of causes and our general knowledge of the combinations, or succession of combinations, in which the phenomena of nature are presented to us, or found to occur in experimental inquiry.

Distinction of Inductive and Deductive Results.

We must carefully avoid confusing together inductive investigations which terminate in the establishment of general laws, and those which seem to lead directly to the knowledge of future particular events. That process only can be called induction which gives general laws, and it is by the subsequent employment of deduction that we can alone anticipate particular events. If the observation of a number of cases shews that alloys of metals fuse at lower temperatures than their constituent metals, I may with more or less probability draw a general inference to that effect, and may thence deductively ascertain the probability that the next alloy examined will fuse at a lower temperature than its constituents. It has been asserted, indeed, by Mr. J. S. Mill[i], and partially admitted by Mr. Fowler[k], that we can argue directly from case to case, so that what is true of some alloys will be true of the next. Doubtless, this is the usual result of our reasoning, regard being had to degrees of probability; but these logicians fail entirely to give any explanation of the process by which we get from case to case. To point, as Mr. Mill has done, to the reasoning, if such it can be called, of brute animals, is little better than to parody philosophy[l]. It may well be allowed, indeed, that the knowledge of future particular events is one main purpose of our investigations, and if there were any process of thought by which we could pass directly from event to event without ascending into general truths, this method would be sufficient, and certainly the most brief and simple. It is true, also, that the laws, of mental association lead the mind always to expect the like again in apparently like circumstances, and even animals of very low intelligence must have some trace of such powers of association, serving to guide them more or less correctly, in the absence of true reasoning faculties. But it is the very purpose of logic, according to Mr. Mill, to ascertain whether inferences have been correctly drawn, rather than to discover them[m]. Even if we can, then, by habit, association, or any rude process of inference, infer the future directly from the past, it is the work of logic to analyse the conditions on which the correctness of this inference depends. Even Mr. Mill would admit that such analysis involves the consideration of general truths[n], and in this, as in several other important points, we might controvert Mr. Mill's own views by his own statements.

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[h] Leslie, 'Inquiry into the Nature of Heat,' Note xvi. p. 521.

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[i] 'System of Logic,' bk. II. chap. iii. Mr. Bain has not adopted the views of Mr. Mill, on this particular point, so far as I can ascertain. See his 'Inductive Logic,' p. 1.

[k] 'Inductive Logic,' pp. 13–14.

[l] 'System of Logic,' bk. II. chap. 3, § 3. Fifth ed. pp. 212–213.

[m] Ibid., Introduction, § 4. Fifth ed. pp. 8–9.

On the Grounds of Inductive Inference.

I hold that, in all cases of inductive inference, we must invent hypotheses, until we fall upon some hypothesis which yields deductive results in accordance with experience. Such accordance renders the chosen hypothesis more or less probable, and we may then deduce, with some degree of likelihood, the nature of our future experience, on the assumption that no arbitrary change takes place in the conditions of nature. We can only argue from the past to the future, on the general principle set forth in the commencement of this work, that what is true of a thing will be true of the like. So far then as one object or event differs from another, all inference is impossible; particulars as particulars can no more make an inference than grains of sand can make a rope. We must always rise to something which is general or same in the cases, and assuming that sameness to be extended to new cases we learn their nature. Hearing a clock tick five thousand times without exception or variation, we adopt the very probable hypothesis that there is some invariably acting machine which produces those uniform sounds, and which will, in the absence of change, go on producing them. Meeting twenty times with a bright yellow ductile substance, and finding it to be always very heavy and incorrodible, I infer that there was some natural condition, which tended, in the creation of things, to associate these properties together, and I expect to find them associated in the next instance. But there always is the possibility that some unknown change may take place between past and future cases. The clock may run down, or be subject to any one of a hundred accidents altering its condition. There is no reason in the nature of things, so far as known to us, why yellow colour, ductility, high specific gravity, and incorrodibility, should always be associated together; and in other like cases, if not in this, men's expectations have been deceived. Our inferences, therefore, always retain more or less of a hypothetical character, and are so far open to doubt. Only in proportion as our induction approximates to the character of perfect induction, does it approximate to certainty. The amount of uncertainty corresponds to the probability that other objects than those examined, may exist and falsify our inferences; the amount of probability corresponds to the amount of information yielded by our examination; and the theory of probability will be needed to prevent our over-estimating or under-estimating the knowledge we possess.

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[n] 'System of Logic,' bk. II. chap. iii. § 5. pp. 225, &c.

Illustrations of the Inductive Process.

To illustrate the passage from the known to the apparently unknown, let us suppose that the phenomena under investigation consist of numbers, and that the following six numbers being exhibited to us, we are required to infer the character of the next in the series:—

5, 15, 35, 45, 65, 95.

The question first of all arises, How may we describe this series of numbers? What is uniformly true of them? The reader cannot fail to perceive at the first glance that they all end in five, and the problem is, from the properties of these six numbers, to infer the properties of the next number ending in five. If we proceed to test their properties by the process of perfect induction, we soon perceive that they have another common property, namely that of being *divisible by five without remainder.* May we then assert that the next number ending in five is also divisible by five, and, if so, upon what grounds? Or extending the question, Is every number ending in five divisible by five? Does it follow that because six numbers obey a supposed law, therefore 376,685,975 or any other number, however large, obeys the law? I answer *certainly not.* The law in question is undoubtedly true; but its truth is not proved by any finite number of examples. All that these six numbers can do, is to suggest to my mind the possible existence of such a law; and I then ascertain its truth, by proving deductively from the rules of decimal numeration, that any number ending in five must be made up of multiples of five, and must therefore be itself a multiple.

To make this more plain, let the reader now examine the numbers—

7, 17, 37, 47, 67, 97.

They all obviously end in 7 instead of 5, and though not at equal intervals, the intervals are exactly the same as in the previous case. After a little consideration, the reader will perceive that these numbers all agree in being *prime numbers,* or multiples of unity only. May we then infer that the next, or any other number ending in 7, is a prime number? Clearly not, for on trial we find that 27, 57, 117 are not primes. Six instances, then, treated empirically, lead us to a true and universal law in one case, and mislead us in another case. We ought, in fact to have no confidence in any law until we have treated it deductively, and have shown that from the conditions supposed the results expected must ensue. From the principles of number, no one can show that numbers ending in 7 should be primes.

From the history of the theory of numbers some good examples of false induction can be adduced. Taking the following series of prime numbers

41, 43, 47, 53, 61, 71, 83, 97, 113, 131, 151, &c.,

it will be found that they all agree in being values of the general expression $x^2 + x + 41$, putting for x in succession the values, 0, 1, 2, 3, 4, &c. We thus seem always to obtain a prime number, and the induction is apparently very strong, to the effect that this expression always will give primes. Yet a few more trials will disprove this false conclusion. Put $x = 40$, and we obtain $40 \times 40 + 40 + 41$, or 41×41. Now such a failure could never have happened, had we shown any deductive reason why $x^2 + x + 41$ should give primes.

There can be no doubt that what here happens with forty instances, might happen with forty thousand or forty million instances. An apparent law never once failing up to a certain point may then suddenly break down, so that inductive reasoning, as it has been described by some writers, can give no sure knowledge of what is to come. Mr. Babbage admirably pointed out, in his Ninth Bridgewater Treatise, that a machine could be constructed to give a perfectly regular series of numbers, through a vast series of steps, and yet to break the law of progression suddenly at any required point. No number of particular cases as particulars enables us to pass by inference to any new case. It is hardly needful to inquire here what can be inferred from an infinite series of facts, because they are never practically within our power; but we may unhesitatingly accept the conclusion, that no finite number of instances can ever prove a general law, or can give us sure knowledge of even one other instance.

General mathematical theorems have indeed been dis-supposed act of induction, that it would succeed in all other cases. Professor de Morgan has recorded a proposed mode of trisecting the angle which could not be discriminated by the senses from a true general solution, except when it was applied to very obtuse angles.[s] In all such cases, it has always turned out either that the angle was not trisected at all, or that only certain particular angles could be thus trisected. They were misled by some apparent or special coincidence, and only deductive proof could establish the truth and generality of the result. In this case, deductive proof shows that the problem, as attempted, is impossible, and that angles generally cannot be trisected by common geometrical methods.

Geometrical Reasoning.

This view of the matter is strongly supported by the further consideration of geometrical reasoning. No skill and care could ever enable us to verify absolutely any one geometrical proposition. Rousseau, in his *Emile*,[t] tells us that we should teach a child geometry by causing him to measure and compare figures by superposition. While a child was yet incapable of general reasoning, this would doubtless be an instructive exercise ; but it never could teach geometry, nor prove the truth of any one proposition. All our figures are rude approximations, and they may happen to seem unequal when they should be equal, and equal when they should be unequal. Moreover, figures may from chance be equal in case after case, and yet there may be no general reason why they should be so. The results of deductive geometrical reasoning are absolutely certain, and are either exactly true or capable of being carried to any required degree of approximation. In a perfect triangle, the angles must be equal to one half-revolution precisely; even an infinitesimal divergence would be impossible; and I believe with equal confidence, that however many are the angles of a figure, provided there are no re-entrant angles, the sum of the angles will be precisely and absolutely equal to twice as many right-angles as the figure has sides, less by four right-angles. In such cases, the deductive proof is absolute and complete; empirical verification can at the most guard against accidental oversights.

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[s] 'Budget of Paradoxes,' p. 257.

[t] 12mo. Amsterdam, 1762, vol. i. p. 401.

There is a second class of geometrical truths which can only be proved by approximation; but, as the mind sees no reason why that approximation should not always go on, we arrive at complete conviction. We thus learn that the surface of a sphere is equal exactly to two-thirds of the whole surface of the circumscribing cylinder, or to four times the area of the generating circle. The area of a parabola is exactly two-thirds of that of the circumscribing parallelogram. The area of the cycloid is exactly three times that of the generating circle. These are truths that we could never ascertain, nor even verify by observation; for any finite amount of difference, vastly less than what the senses can discern, would falsify them. There are again geometrical relations which we cannot assign exactly, but can carry to any desirable degree of approximation. Thus, the ratio of the circumference to the diameter of a circle is that of 3·14159265358979323846.... to 1, and the approximation may be carried to any extent by the expenditure of sufficient labour, as many as 607 places of figures having been calculated.[u] Some years since, I amused myself by trying how near I could get to this ratio, by the careful use of compasses, and I did not come

[u] 'English Cyclopædia,' art. *Tables*.

discontinuous quantity or numbers, however, allow of certainty; for I may establish beyond doubt, for instance, that the difference of the squares of 17 and 13 is the product of $17 + 13$ and $17 - 13$, and is therefore 30×4, or 120.

Inferences which we draw concerning natural objects are never certain except in a hypothetical point of view. It might seem, indeed, to be certain that iron is magnetic, or that gold is incapable of solution in nitric acid; but, if we carefully investigate the meanings of these statements, they will be found to involve no certainty but that of consciousness and that of hypothetical inference. For what do I mean by iron or gold? If I choose a remarkable piece of yellow substance, call it gold, and then immerse it in a liquid which I call nitric acid, and find that there is no change called solution, then consciousness has certainly informed me that with my meaning of the terms, 'Gold is insoluble in nitric acid.' I may further be certain of something else; for if this gold and nitric acid remain what they were, I may be sure there will be no solution on again trying the experiment. If I take other portions of gold and nitric acid, and am sure that they really are identical in properties with the former portions, I can be certain that there will be no solution. But at this point my knowledge becomes purely hypothetical; for how can I be sure without trial that the gold and acid are really identical in nature with what I formerly called gold and nitric acid. How do I know gold when I see it? If I judge by the apparent qualities—colour, ductility, specific gravity, &c., I may be misled, because there may always exist a substance which to the colour, ductility, specific gravity, and other specified qualities, joins others which we do not expect. Similarly, if iron is magnetic, as shown by an experiment with objects answering to those names, then all iron is magnetic, meaning all pieces of matter identical with my assumed piece. But in trying to identify iron, I am always open to mistake. Nor is this liability to mistake a matter of speculation only[v].

The history of chemistry shows that the most confident inferences may have been falsified by the confusion of one substance with another. Thus strontia was never discriminated from baryta until Klaproth and Haüy detected differences between some of their properties[x]. Accordingly chemists must often have inferred concerning strontia what was only true of baryta, and *vice versâ*. There is now no doubt that the recently discovered substances, cæsium and rubidium were long mistaken for potassium[y]. Other elements have often been confused together, for instance, tantalum and niobium; sulphur and selenium; cerium, lanthanum, and didymium; yttrium and erbium.

Even the best-established laws of physical science do not exclude false inference. No law of nature has been better established than that of universal gravitation, and we believe with the utmost confidence that any body capable of affecting the senses will attract other bodies, and fall to the earth if not prevented. Euler remarks that, although he had never made trial of the stones which compose the church of Magdeburg, yet he had not the least doubt that all of them were heavy, and would fall if unsupported. But he adds, that it would be extremely difficult to give any satisfactory explanation of this confident belief[z]. The fact is, that the belief ought not to amount to certainty until the experiment has been tried, and in the meantime a slight amount of uncertainty enters, because we cannot be sure that the stones of the Magdeburg church resemble other stones in all their properties.

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[v] Professor Bowen has excellently stated this view. 'Treatise on Logic.' Cambridge, U.S.A., 1866. P. 354.

[x] Whewell's 'History of the Inductive Sciences,' vol. iii. p. 174.

[y] Roscoe's 'Spectrum Analysis,' 1st edit. p. 99.

[z] Euler's 'Letters to a German Princess,' translated by Hunter, 2nd ed. vol. ii. pp. 17–18.

In like manner, not one of the inductive truths which men have established, or think they have established, is really safe from exception or reversal. Lavoisier, when laying the foundations of chemistry, met with so many instances tending to show the existence of oxygen in all acids, that he adopted a general conclusion to that effect, and devised the name oxygen accordingly. He entertained no appreciable doubt that the acid existing in sea salt also contained oxygen[a]; yet subsequent experience falsified his expectations.

This instance refers to a science in its infancy, speaking relatively to the possible achievements of men. But all sciences are and will ever remain in their infancy, relatively to the extent and complexity of the universe which they undertake to investigate. Euler expresses no more than the truth when he says that it would be impossible to fix on any one thing really existing, of which we could have so perfect a knowledge as to put us beyond the reach of mistake[b].

Like remarks may be made concerning all other inductive inferences. We may be quite certain that a comet will go on moving in a similar path *if* all circumstances remain the same as before; but if we leave out this extensive qualification, our predictions will always be subject to the chance of falsification by some wholly unexpected event, such as the division of Biela's comet, or the unforeseen interference of some planetary or other gravitating body.

Inductive inference might attain to certainty if our

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[a] Lavoisier's 'Chemistry,' translated by Kerr. 3rd edit. pp. 114, 121, 123.

[b] Euler's 'Letters,' vol. ii. p. 21.

CHAPTER XII.

THE INDUCTIVE OR INVERSE APPLICATION OF THE THEORY OF PROBABILITIES.

We have hitherto considered the theory of probability only in its simple deductive employment, by which it enables us to determine from given conditions the probable character of events happening under those conditions. But as deductive reasoning when inversely applied constitutes the process of induction, so the calculation of probabilities may be inversely applied; from the known character of certain events we may argue backwards to the probability of a certain law or condition governing those events. Having satisfactorily accomplished this work, we may indeed calculate forwards to the probable character of future events happening under the same conditions; but this part of the process is a direct use of deductive reasoning (p. 260).

Now it is highly instructive to find that whether the theory of probabilities be deductively or inductively applied, the calculation is always performed according to the principles and rules of deduction. The probability that an event has a particular condition entirely depends upon the probability that if the condition existed the event would follow. If we take up a pack of common playing cards, and observe that they are arranged in perfect numerical order, we conclude beyond all reasonable doubt that they have been thus intentionally arranged by some person acquainted with the usual order of sequence. This conclusion is quite irresistible, and rightly so; for there are but two suppositions which we can make as to the reason of the cards being in that particular order:—

1. They have been intentionally arranged by some one who would probably prefer the numerical order.

2. They have fallen into that order by chance, that is, by some series of conditions which, being wholly unknown in nature, cannot be known to lead by preference to the particular order in question.

The latter supposition is by no means absurd, for any one order is as likely as any other when there is no preponderating tendency. But we can readily calculate by the doctrines of permutation the probability that fifty-two objects would fall by chance into any one particular order. Fifty-two objects can be arranged in—

$$52 \times 51 \times 50 \times \ldots \times 4 \times 3 \times 2 \times 1 \text{ or } 8066 \times (10)^{64}$$

possible orders, the number obtained requiring 68 places of figures for its full expression. Hence it is excessively unlikely, and, in fact, practically impossible, that any one should ever meet with a pack of cards arranged in perfect order by pure accident. If we do meet with a pack so arranged, we inevitably adopt the other supposition, that some person having reasons for preferring that special order, has thus put them together.

We know that of the almost infinite number of possible orders the numerical order is the most remarkable; it is useful as proving the perfect constitution of the pack, and it is the intentional result of certain games. At any rate, the probability that intention should produce that order is incomparably greater than the probability that chance should produce it; and as a certain pack exists in that order, we rightly prefer the supposition which most probably leads to the observed result.

By a similar mode of reasoning we every day arrive, and validly arrive, at conclusions approximating to certainty. Whenever we observe a perfect resemblance between two objects, as, for instance, two printed pages, two engravings, two coins, two foot-prints, we are warranted in asserting that they proceed from the same type, the same plate, the same pair of dies, or the same boot. And why? Because it is almost impossible that with different types, plates, dies, or boots some minute distinction of form should not be discovered. It is barely possible for the hand of the most skilful artist to make two objects alike, so that mechanical repetition is the only probable explanation of exact similarity. We can often establish with extreme probability that one document is copied from another. Suppose that each document contains 10,000 words, and that the same word is incorrectly spelt in each. There is then a probability of less than 1 in 10,000 that the same mistake should be made in each.

If we meet with a second error occurring in each document, the probability is less than 1 in 10,000 × 9999, that such two coincidences should occur by chance, and the numbers grow with extreme rapidity for more numerous coincidences. We cannot indeed make any precise calculations without taking into account the character of the errors committed, concerning the conditions of which we have no accurate means of estimating probabilities. Nevertheless, abundant evidence may thus be obtained as to the derivation of documents from each other. In the examination of many sets of logarithmic tables, six remarkable errors were found to be present in all but two, and it was proved that tables printed at Paris, Berlin, Florence, Avignon, and even in China, besides thirteen sets printed in England, between the years 1633 and 1822, were derived directly or indirectly from some common source[a]. With a certain amount of labour, it is possible to establish beyond reasonable doubt the relationship or genealogy of any number of copies of one document, proceeding possibly from parent copies now lost. Tischendorf has thus investigated the relations between the manuscripts of the New Testament now existing, and the same work has been performed by German scholars for several classical writings.

Principle of the Inverse Method.

The inverse application of the rules of probability entirely depends upon a proposition which may be thus stated, nearly in the words of Laplace[b]. *If an event can be produced by any one of a certain number of different causes, the probabilities of the existence of these causes as inferred from the event, are proportional to the probabilities of the event as derived from these causes.* In other words, the most probable cause of an event which has happened is that which would most probably lead to the event supposing the cause to exist; but all other possible causes are also to be taken into account with probabilities proportional to the probability that the event would have happened if the cause existed. Suppose, to fix our ideas clearly, that E is the event, and C_1 C_2 C_3 are the three only conceivable causes. If C_1 exist, the probability is p_1 that E would follow; if C_2 and C_3 exist, the like probabilities are respectively p_2 and p_3. Then as p_1 is to p_2, so is the probability of C_1 being the actual cause to the probability of C_2 being it; and, similarly, as p_2 is to p_3, so is the probability of C_2 being the actual cause to the probability of C_3 being it. By a very simple mathematical process we arrive at the conclusion that the actual probability of C_1 being the cause is

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[a] Lardner, 'Edinburgh Review,' July 1834, p. 277.

[b] 'Mémoires par divers Savans,' tom. vi.; quoted by Todhunter in his 'History of Theory of Probability,' p. 458.

$$\frac{p_1}{p_1 + p_2 + p_3};$$

and the similar probabilities of the existence of C_2 and C_3 are,

$$\frac{p_2}{p_1 + p_2 + p_3} \text{ and } \frac{p_3}{p_1 + p_2 + p_3}.$$

The sum of these three fractions amounts to unity, which correctly expresses the certainty that one cause or other must be in operation.

We may thus state the result in general language. If it is certain that one or other of the supposed causes exists, the probability that any one does exist is the probability that if it exists the event happens, divided by the sum of all the similar probabilities. There may seem to be an intricacy in this subject which may prove distasteful to some readers; but this intricacy is essential to the subject in hand. No one can possibly understand the principles of inductive reasoning, unless he will take the trouble to master the meaning of this rule, by which we recede from an event to the probability of each of its possible causes.

This rule or principle of the indirect method is that which common sense leads us to adopt almost instinctively, before we have any comprehension of the principle in its general form. It is easy to see, too, that it is the rule which will, out of a great multitude of cases, lead us most often to the truth, since the most probable cause of an event really means that cause which in the greatest number of cases produces the event. But I have only met with one attempt at a general demonstration of the principle. Poisson imagines each possible cause of an event to be represented by a distinct ballot-box, containing black and white balls, in such ratio that the probability of a white ball being drawn is equal to that of the event happening. He further supposes that each box, as is possible, contains the same total number of balls, black and white; and then, mixing all the contents of the boxes together, he shows that if a white ball be drawn from the aggregate ballot-box thus formed, the probability that it proceeded from any particular ballot-box is represented by the number of white balls in that particular box, divided by that total number of white balls in all the boxes. This result corresponds to that given by the principle in question[c].

Thus, if there be three boxes, each containing ten balls in all, and respectively containing seven, four, and three white balls, then on mixing all the balls together we have fourteen white ones; and if we draw a white ball, that is if the event happens, the probability that it came out of the first box is $\frac{7}{14}$; which is exactly equal to $\frac{\frac{7}{10}}{\frac{7}{10}+\frac{4}{10}+\frac{3}{10}}$, the fraction given by the rule of the Inverse Method.

Simple Applications of the Inverse Method.

In many cases of scientific induction we may apply the principle of the inverse method in a simple manner. If only two, or at the most a few hypotheses, may be made as to the origin of certain phenomena, or the connection of one phenomenon with another, we may sometimes easily calculate the respective probabilities of these hypotheses. It was thus that Professors Bunsen and Kirchhoff established, with a probability little short of certainty, that iron exists in the sun. On comparing the spectra of sunlight and of the light proceeding from the incandescent vapour of iron, it became apparent that at least sixty bright lines in the spectrum of iron coincided with dark lines in the sun's spectrum. Such coincidences could never be observed with certainty, because, even if the lines only closely approached, the instrumental imperfections of the spectroscope would make them apparently coincident, and if one line came within half a millemetre of another, on the map of the spectra, they could not be pronounced distinct. Now the average distance of the solar lines on Kirchhoff's map is 2 millemetres, and if we throw down a line, as it were, by pure chance on such a map, the probability is about one-half that the new line will fall within $\frac{1}{2}$ millemetre on one side or the other of some one of the solar lines. To put it in another way, we may suppose that each solar line, either on account of its real breadth or the defects of the instrument, possesses a breadth of $\frac{1}{2}$ millemetre, and that each line in the iron spectrum has a like breadth. The probability then is just one-half that the centre of each iron line will come by chance within 1 millemetre of the centre of a solar line, so as to appear to coincide with it. The probability of casual coincidence of each iron line with a solar line is in like manner $\frac{1}{2}$. Coincidence in the case of each of the sixty iron lines is a very unlikely event if it arises casually, for it would have a probability of only $(\frac{1}{2})^{60}$ or less than 1 in a trillion. The odds, in short, are more than a million million millions to unity against such casual coincidence[d]. But on the other hypothesis, that iron exists in the sun, it is highly probable that such coincidences would be observed; it is immensely more probable that sixty coincidences would be observed if iron existed in the sun, than that they should arise from chance. Hence by our principle it is immensely probable that iron does exist in the sun.

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[c] Poisson, 'Recherches sur la Probabilité des Jugements,' Paris, 1837, pp. 82, 83.

All the other interesting results given by the comparison of spectra, rest upon the same principle of probability. The almost complete coincidence between the spectra of solar, lunar, and planetary light renders it practically certain that the light is all of solar origin, and is reflected from the surfaces of the moon and planets, suffering only slight alteration from the atmospheres of some of the planets. A fresh confirmation of the truth of the Copernican theory is thus furnished.

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[d] Kirchhoff's 'Researches on the Solar Spectrum.' First part, translated by Professor Roscoe, pp. 18, 19.

A vast probability may be shown to exist that the heat, light, and chemical effects of the sun are due to the same rays, and are so many different manifestations of the same undulations. For a photograph of the spectrum corresponds exactly with what the eye observes, allowance being made for the great differences of chemical activity in different parts of the spectrum; and delicate experiments with the thermopile also show that, where there is a dark line, there also the heat of the rays is absent.

Sir J. Herschel proved the connexion between the direction of the oblique faces of symmetrical quartz crystals, and the direction in which the same crystals rotate the plane of the polarisation of light. For if it is found in a second crystal that the relation is the same as in the first, the probability of this happening by chance is $\frac{1}{2}$; the probability that in another crystal also the direction would be the same is $\frac{1}{4}$, and so on. The probability that in $n + 1$ crystals there would be casual agreement of direction is the n^{th} power of $\frac{1}{2}$. Thus, if in examining fourteen crystals the same relation of the two phenomena is discovered in each, the probability that it proceeds from uniform conditions is more than 8000 to 1 [*]. Now, since the first observations on this subject were made in 1820, no exceptions have been observed, so that the probability of invariable connexion is incalculably great.

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[*] 'Edinburgh Review,' No. 185, vol. xcii. July 1850, p. 32; Herschel's 'Essays,' p. 421; 'Transactions of the Cambridge Philosophical Society,' vol. i. p. 43.

A good instance of this method is furnished by the agreement of numerical statements with the truth. Thus, in a manuscript of Diodorus Siculus, as Dr. Young states[g], the ceremony of an ancient Egyptian funeral is described as requiring the presence of forty-two persons sitting in judgment on the merits of the deceased, and in many ancient papyrus rolls the same number of persons are found delineated. The probability is but slight that Diodorus, if inventing his statements or writing without proper information, would have chosen such a number as forty-two, and though there are not the data for an exact calculation, Dr. Young considers that the probability in favour of the correctness of the manuscript and the veracity of the writer on this ground alone, is at least 100 to 1.

It is exceedingly probable that the ancient Egyptians had exactly recorded the eclipses occurring during long periods of time, for Diogenes Laertius mentions that 373 solar and 832 lunar eclipses had been observed, and the ratio between these numbers exactly expresses that which would hold true of the eclipses of any long period, of say 1200 or 1300 years, as estimated on astronomical grounds[h].

It is evident that an agreement between small numbers, or customary numbers, such as seven, one hundred, a myriad, &c., is much more likely to happen from chance, and therefore gives much less presumption of dependence. If two ancient writers spoke of the sacrifice of oxen, they would in all probability describe it as a hecatomb, and there would be nothing remarkable in the coincidence.

On similar grounds, we must inevitably believe in the human origin of the flint flakes so copiously discovered of late years. For though the accidental stroke of one stone against another may often produce flakes, such as are occasionally found on the sea-shore, yet when several flakes are found in close company, and each one bears evidence, not of a single blow only, but of several successive blows, all conducing to form a symmetrical knife-like form, the probability of a natural and accidental origin becomes incredibly small, and the contrary supposition, that they are the work of intelligent beings, approximately certain[i].

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[g] Young's 'Works,' vol. ii. pp. 18, 19.

[h] 'History of Astronomy,' Library of Useful Knowledge, p. 14.

An interesting calculation concerning the probable connexion of languages, in which several or many words are similar in sound and meaning, was made by Dr. Young[k].

Application of the Theory of Probabilities in Astronomy.

The science of astronomy, occupied with the simple relations of distance, magnitude, and motion of the heavenly bodies, admits more easily than almost any other science of interesting conclusions founded on the theory of probability. More than a century ago, in 1767, Michell showed the extreme probability of bonds connecting together systems of stars. He was struck by the unexpected number of fixed stars which have companions close to them. Such a conjunction might happen casually by one star, although possibly at a great distance from the other, happening to lie on the same straight line passing near the earth. But the probabilities are so greatly against such an optical union happening often in the expanse of the heavens, that Michell asserted the existence of a bond between most of the double stars. It has since been estimated by Struve, that the odds are 9570 to 1 against any two stars of not less than the seventh magnitude falling within the apparent distance of four seconds of each other by chance, and yet ninety-one such cases were known when the estimation was made, and many more cases have since been discovered. There were also four known triple stars, and yet the odds against the appearance of any one such conjunction are 173,524 to 1[l]. The conclusions of Michell have been entirely verified by the discovery that many double stars are in connexion under the law of gravitation.

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[i] Evans' 'Ancient Stone Implements of Great Britain.' London, 1872 (Longmans).

[k] 'Philosophical Transactions,' 1819; Young's 'Works,' vol. ii. pp. 15-18.

Michell also investigated the probability that the six brightest stars in the Pleiades should have come by accident into such striking proximity. Estimating the number of stars of equal or greater brightness at 1500, he found the odds to be nearly 500,000 to 1 against casual conjunction. Extending the same kind of argument to other clusters, such as that of Præsepe, the nebula in the hilt of Perseus' sword, he says[m]: 'We may with the highest probability conclude, the odds against the contrary opinion being many million millions to one, that the stars are really collected together in clusters in some places, where they form a kind of system, while in others there are either few or none of them, to whatever cause this may be owing, whether to their mutual gravitation, or to some other law or appointment of the Creator.'

The calculations of Michell have been called in question by the late James D. Forbes[n], and Mr. Todhunter vaguely countenances his objections[o], otherwise I should not have thought them of much weight. Certainly Laplace accepts Michell's views[p], and if Michell be in error, it is in the methods of calculation, not in the general validity of his conclusions.

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[l] Herschel, 'Outlines of Astronomy,' 1849, p. 565; but Todhunter, in his 'History of the Theory of Probability,' p. 335, states that the calculations do not agree with those published by Struve.

[m] 'Philosophical Transactions,' 1767, vol. lvii. p. 431.

[n] 'Philosophical Magazine,' 3rd Series, vol. xxxvii. p. 401, December, 1850; also August, 1849.

Similar calculations might no doubt be applied to the peculiar drifting motions which have been detected by Mr. R. A. Proctor in some of the constellations[q]. Against a general tendency of stars to move in one direction by chance, the odds are very great. It is on a similar ground that a considerable proper motion of the sun is found to exist with immense probability, because on the average the fixed stars show a tendency to move apparently from one point of the heavens towards that diametrically opposite. The sun's motion in the contrary direction would explain this tendency, otherwise we must believe that myriads of stars accidentally agree in their direction of motion, or are urged by some common force from which the sun is exempt. It may be said that the rotation of the earth is proved in like manner, because it is immensely more probable that one body would revolve than that the sun, moon, planets, comets, and the whole of the stars of the heavens should be whirled round the earth daily, with a uniform motion superadded to their own peculiar motions. This appears to be nearly the reason which led Gilbert, one of the earliest English Copernicans, and in every way an admirable physicist, to admit the rotation of the earth, while Francis Bacon denied it[r].

In contemplating the planetary system, we are struck with the similarity in direction of nearly all its movements. Newton remarked upon the regularity and uniformity of these motions, and contrasted them with the eccentricity and irregularity of the cometary orbits[s]. Could we, in fact, look down upon the system from the northern side, we should see all the planets moving round from west to east, the satellites moving round their primaries and the sun, planets, and all the satellites rotating in the same direction, with some exceptions on the verge of the system. Now in the time of Laplace eleven planets were known, and the directions of rotation were known for the sun, six planets, the satellites of Jupiter, Saturn's ring, and one of his satellites. Thus there were altogether 43 motions all concurring, namely :—

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[o] 'History,' &c., p. 334.

[p] 'Essai Philosophique,' p. 57.

[q] 'Proceedings of the Royal Society,' 20 January, 1870. 'Philosophical Magazine,' 4th Series, vol. xxxix. p. 381.

[r] Hallam's 'Literature of Europe,' 1st ed. vol. ii. p. 464.

Orbital motions of eleven planets . .	11
Orbital motions of eighteen satellites . .	18
Axial rotations	14
	43

The probability that 43 motions independent of each other would coincide by chance is the 42nd power of $\frac{1}{2}$, so that the odds are about 4,400,000,000,000 to 1 in favour of some common cause for the uniformity of direction. This probability, as Laplace observes[t], is higher than that of many historical events which we undoubtingly believe. In the present day, the probability is much increased by the discovery of additional planets, and the rotation of other satellites, and it is only slightly weakened by the fact that some of the outlying satellites are exceptional in direction, there being considerable evidence of an accidental disturbance in the more distant parts of the system.

Hardly less remarkable than the uniformity of motion is the near approximation of all the orbits of the planets to a common plane. Daniel Bernouilli roughly estimated the probability of such an agreement arising from accident at $\frac{1}{(12)^6}$, the greatest inclination of any orbit to the sun's equator being 1-12th part of a quadrant. Laplace devoted to this subject some of his most ingenious investigations. He found the probability that the sum of the inclinations of the planetary orbits would not exceed by accident the actual amount (·914187 of a right angle for the ten planets known in 1801) to be $\frac{1}{10}(\cdot 914187)^{10}$, or about ·00000011235. This probability may be combined with that derived from the direction of motion, and it then becomes immensely probable that the constitution of the planetary system arose out of uniform conditions, or, as we say, from some common cause[u].

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[s] 'Principia,' bk. ii. General scholium.

[t] 'Essai Philosophique,' p. 55. Laplace appears to count the rings of Saturn as giving two independent movements.

If the same kind of calculation be applied to the orbits of comets the result is very different[y]. Of the orbits which have been determined 48·9 per cent. only are direct or in the same direction as the planetary motions[z]. Hence it becomes apparent that comets do not properly belong to the solar system, and it is probable that they are stray portions of nebulous matter which have become accidently attached to the system by the attractive powers of the sun or Jupiter.

Statement of the General Inverse Problem.

In the instances described in the preceding sections, we have been occupied in receding from the occurrence of certain similar events to the probability that there must have been a condition or cause for such events. We have found that the theory of probability, although never yielding a certain result, often enables us to establish an hypothesis beyond the reach of reasonable doubt. There is, however, another method of applying the theory, which possesses for us even greater interest, because it illustrates, in the most complete manner, the theory of inference adopted in this work, which theory indeed it suggested. The problem to be solved is as follows :—

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[u] Lubbock, 'Essay on Probability,' p. 14. De Morgan, 'Encyc. Metrop.' art. *Probability*, p. 412. Todhunter's 'History of the Theory of Probability,' p. 543. Concerning the objections raised to these conclusions by the late Dr. Boole, see the 'Philosophical Magazine,' 4th Series, vol. ii. p. 98. Boole's 'Laws of Thought,' pp. 364–375.

[y] Laplace, 'Essai Philosophique,' pp. 55, 56.

[z] Chambers's 'Astronomy,' 2nd ed. pp. 346-49.

An event having happened a certain number of times, and failed a certain number of times, required the probability that it will happen any given number of times in the future under the same circumstances.

All the larger planets hitherto discovered move in one direction round the sun; what is the probability that, if a new planet exterior to Neptune be discovered, it will move in the same direction? All known permanent gases, except chlorine, are colourless; what is the probability that, if some new permanent gas should be discovered, it will be colourless? In the general solution of this problem, we wish to infer the future happening of any event from the number of times that it has already been observed to happen. Now, it is very instructive to find that there is no known process by which we can pass directly from the data to the conclusion. It is always requisite to recede from the data to the probability of some hypothesis, and to make that hypothesis the ground of our inference concerning future happenings. Mathematicians, in fact, make every hypothesis which is applicable to the question in hand; they then calculate, by the inverse method, the probability of every such hypothesis according to the data, and the probability that if each hypothesis be true, the required future event will happen. The total probability that the event will happen, is the sum of the separate probabilities contributed by each distinct hypothesis.

To illustrate more precisely the method of solving the problem, it is desirable to adopt some concrete mode of representation, and the ballot-box, so often employed by mathematicians, will best serve our purpose. Let the happening of any event be represented by the drawing of a white ball from a ballot-box, while the failure of an event is represented by the drawing of a black ball. Now, in the inductive problem we are supposed to be ignorant of the contents of the ballot-box, and are required to ground all our inferences on our experience of those contents as shown in successive drawings. Rude common sense would guide us nearly to a true conclusion. Thus if we had drawn twenty balls, one after another, replacing the ball after each drawing, and the ball had in each case proved to be white, we should believe that there was a considerable preponderance of white balls in the urn, and a probability in favour of drawing a white ball on the next occasion. Though we had drawn white balls for thousands of times without fail, it would still be possible that some black balls lurked in the urn and would at last appear, so that our inferences could never be certain. On the other hand, if black balls came at intervals, I should expect that after a certain number of tiials the future results would agree more or less closely with the past ones.

The mathematical solution of the question consists in nothing more than a close analysis of the mode in which our common sense proceeds. If twenty white balls have been drawn and no black ball, my common sense tells me that any hypothesis which makes the black balls in the urn considerable compared with the white ones is improbable; a preponderance of white balls is a more probable hypothesis, and as a deduction from this more probable hypothesis, I expect a recurrence of white balls. The mathematician merely reduces this process of thought to exact numbers. Taking, for instance, the hypothesis that there are 99 white and one black ball in the urn, he can calculate the probability that 20 white balls should be drawn in succession in those circumstances; he thus forms a definite estimate of the probability of this hypothesis, and knowing at the same time the probability of a white ball reappearing if such be the contents of the urn, he combines these probabilities, and obtains an exact estimate that a white ball will recur in consequence of this hypothesis. But as this hypothesis is only one out of many possible ones, since the ratio of white and black balls may be 98 to 2, or 97 to 3, or 96 to 4, and so on, he has to repeat the estimate for every such possible hypothesis. To make the method of solving the problem perfectly evident, I will describe in the next section a very simple case of the problem, originally devised for the purpose by Condorcet, which was also adopted by Lacroix[a], and has passed into the works of De Morgan, Lubbock, and others.

Simple Illustration of the Inverse Problem.

Suppose it to be known that a ballot-box contains only four black or white balls, the ratio of black and white balls being unknown. Four drawings having been made with replacement, and a white ball having appeared on each occasion but one, it is required to determine the probability that a white ball will appear next time. Now the hypotheses which can be made as to the contents of the urn are very limited in number, and are at most the following five:—

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[a] 'Traité élémentaire du Calcul des Probabilités,' 3rd ed. (1833), p. 148.

4 white and 0 black balls
3 ” ” 1 ” ”
2 ” ” 2 ” ”
1 ” ” 3 ” ”
0 ” ” 4 ” ”

The actual occurrence of black and white balls in the drawings renders the first and last hypotheses out of the question, so that we have only three left to consider.

If the box contains three white and one black, the probability of drawing a white each time is $\frac{3}{4}$, and a black $\frac{1}{4}$; so that the compound event observed, namely, three white and one black, has the probability $\frac{3}{4} \times \frac{3}{4} \times \frac{3}{4} \times \frac{1}{4}$, by the rule already given (p. 233). But as it is indifferent to us in what order the balls are drawn, and the black ball might come first, second, third, or fourth, we must multiply by four, to obtain the probability of three white and one black in any order, thus getting $\frac{27}{64}$.

Taking the next hypothesis of two white and two black balls in the urn, we obtain for the same probability the quantity $\frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times 4$, or $\frac{16}{64}$, and from the third hypothesis of one white and three black we deduce likewise $\frac{1}{4} \times \frac{1}{4} \times \frac{1}{4} \times \frac{3}{4} \times 4$, or $\frac{3}{64}$. According, then, as we adopt the first, second, or third hypothesis, the probability that the result actually noticed would follow is $\frac{27}{64}$, $\frac{16}{64}$, and $\frac{3}{64}$. Now it is certain that one or other of these hypotheses must be the true one, and their absolute probabilities are proportional to the probabilities that the observed events would follow from them (see p. 279). All we have to do, then, in order to obtain the absolute probability of each hypothesis, is to alter these fractions in a uniform ratio, so that their sum shall be unity, the expression of certainty. Now since $27 + 16 + 3 = 46$, this will be effected by dividing each fraction by 46 and multiplying by 64. Thus the probabilities of the first, second, and third hypotheses are respectively—

$$\frac{27}{46},\quad \frac{16}{46},\quad \frac{3}{46}.$$

The inductive part of the problem is now completed, since we have found that the urn most likely contains three white and one black ball, and have assigned the exact probability of each possible supposition. But we are now in a position to resume deductive reasoning, and infer the probability that the next drawing will yield, say a white ball. For if the box contains three white and one black ball, the probability of drawing a white one is certainly $\frac{3}{4}$; and as the probability of the box being so constituted is $\frac{27}{46}$, the compound probability that the box will be so filled and will give a white ball at the next trial, is

$$\frac{27}{46} \times \frac{3}{4} \text{ or } \frac{81}{184}.$$

Again, the probability is $\frac{16}{46}$ that the box contains two white and two black, and under those conditions the probability is $\frac{1}{2}$ that a white ball will appear; hence the probability that a white ball will appear in consequence of that condition, is

$$\frac{16}{46} \times \frac{1}{2} \text{ or } \frac{32}{184}.$$

From the third supposition we get in like manner the probability

$$\frac{3}{46} \times \frac{1}{4} \text{ or } \frac{3}{184}.$$

Now since one and not more than one hypothesis can be true, we may add together these separate probabilities, and we find that

$$\frac{81}{184} + \frac{32}{184} + \frac{3}{184} \text{ or } \frac{116}{184}$$

is the complete probability that a white ball will be next drawn under the conditions and data supposed.

General Solution of the Inverse Problem.

In the instance of the inverse method described in the last section, a very few balls were supposed to be in the ballot-box for the purpose of simplifying the calculation. In order that our solution may apply to natural phenomena, we must render our hypothesis as little arbitrary as possible. Having no *à priori* knowledge of the conditions of the phenomena in question, there is no limit to the variety of hypotheses which might be suggested. Mathematicians have therefore had recourse to the most extensive suppositions which can be made, namely, that the ballot-box contains an infinite number of balls; they have thus varied the proportion of white balls to black balls continuously, from the smallest to the greatest possible proportion, and estimated the aggregate probability which results from this comprehensive supposition.

To explain their procedure, let us imagine that, instead of an infinite number, the ballot-box contained a large finite number of balls, say 1000. Then the number of white balls might be 1 or 2 or 3 or 4, and so on, up to 999. Supposing that three white and one black ball have been drawn from the urn as before, there is a certain very small probability that this would have occurred in the case of a box containing one white and 999 black balls; there is also a small probability that from such a box the next ball would be white. Compound these probabilities, and we have the probability that the next ball really will be white, in consequence of the existence of that proportion of balls. If there be two white and 998 black balls in the box, the probability is greater, and will increase until the balls are supposed to be in the proportion of those drawn. Now 999 different hypotheses are possible, and the calculation is to be made for each of these, and their aggregate taken as the final result. It is apparent that as the number of balls in the box is increased, the absolute probability of any one hypothesis concerning the exact proportion of balls is decreased, but the aggregate results of all the hypotheses will assume the character of a wide average.

When we take the step of supposing the balls within the urn to be infinite in number, the possible proportions of white and black balls also become infinite, and the probability of any one proportion actually existing is infinitely small. Hence the final result that the next ball drawn will be white is really the sum of an infinite number of infinitely small quantities. It might seem, indeed, utterly impossible to calculate out a problem having an infinite number of hypotheses, but the wonderful resources of the integral calculus enable this to be done with far greater facility than if we supposed any large finite number of balls, and then actually computed the results. I will not attempt to describe the processes by which Laplace finally accomplished the complete solution of the problem. They are to be found described in several English works, especially De Morgan's 'Treatise on Probabilities,' in the 'Encyclopædia Metropolitana,' and Mr. Todhunter's 'History of the Theory of Probability.' The abbreviating power of mathematical analysis was never more strikingly shown. But I may add that though the integral calculus is employed as a means of summing infinitely numerous results, we in no way abandon the principles of combinations already treated. We calculate the values of infinitely numerous factorials, not, however, obtaining their actual products, which would lead to an infinite number of figures, but obtaining the final answer to the problem by devices which can only be comprehended after study of the integral calculus.

It must be allowed that the hypothesis adopted by Laplace is in some degree arbitrary, so that there was some opening for the doubt which Boole has cast upon it[b]. But it may be replied, (1) that the supposition of an infinite number of balls treated in the manner of Laplace is less arbitrary and more comprehensive than any other that could be suggested. (2) The result does not differ much from that which would be obtained on the hypothesis of any very large finite number of balls. (3) The supposition leads to a series of simple formulæ which can be applied with ease in many cases, and which bear all the appearance of truth so far as it can be independently judged by a sound and practiced understanding.

Rules of the Inverse Method.

By the solution of the problem, as described in the last section, we obtain the following series of simple rules.

1. *To find the probability that an event which has not hitherto been observed to fail will happen once more, divide the number of times the event has been observed increased by one, by the same number increased by two.*

If there have been m occasions on which a certain event might have been observed to happen, and it has happened on all those occasions, then the probability that it will happen on the next occasion of the same kind is $\frac{m+1}{m+2}$. For instance, we may say that there are nine places in the planetary system where planets might exist obeying Bode's law of distance, and in every place there is a planet obeying the law more or less exactly, although no reason is known for the coincidence. Hence the probability that the next planet beyond Neptune will conform to the law is $\frac{10}{11}$.

2. *To find the probability that an event which has not hitherto failed will not fail for a certain number of new occasions, divide the number of times the event has hap-*

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[b] 'Laws of Thought,' pp. 368–375.

pened increased by one, by the same number increased by one and the number of times it is to happen.

An event having happened m times without fail, the probability that it will happen n more times is $\frac{m+1}{m+n+1}$. Thus the probability that three new planets would obey Bode's law is $\frac{10}{13}$, but it must be allowed that this, as well as the previous result, would be much weakened by the fact that Neptune can barely be said to obey the law.

3. *An event having happened and failed a certain number of times, to find the probability that it will happen the next time, divide the number of times the event has happened increased by one, by the whole number of times the event has happened or failed increased by two.*

Thus, if an event has happened m times and failed n times, the probability that it will happen on the next occasion is $\frac{m+1}{m+n+2}$.

Thus, if we assume that of the elements yet discovered 50 are metallic and 14 non-metallic, then the probability that the next element discovered will be metallic is $\frac{51}{66}$.

Again since of 37 metals which have been sufficiently examined only four, namely, sodium, potassium, lanthanum and lithium, are of less density than water, the probability that the next metal examined or discovered will be less dense than water is $\frac{4+1}{37+2}$ or $\frac{5}{39}$.

We may state the results of the method in a more general manner thus,—If under given circumstances certain events A, B, C, &c., have happened respectively m, n, p, &c., times, and one or other of these events must happen, then the probabilities of these events are proportional to $m+1$, $n+1$, $p+1$, &c., so that the probability of A will be $\frac{m+1}{m+1+n+1+p+1+ \&c.}$. But if new events may happen in addition to those which have been observed, we must assign unity for the probability of such new event. The proportional probabilities then become 1 for a new event, $m+1$ for A, $n+1$ for B, and so on, and the absolute probability of A is $\frac{m+1}{1+m+1+n+1+\&c.}$[c]

It is very interesting to trace out the variations of probability according to these rules under diverse circumstances. Thus the first time a casual event happens it is 1 to 1, or as likely as not that it will happen again; if it does happen it is 2 to 1 that it will happen a third time; and on successive occasions of the like kind the odds become 3, 4, 5, 6, &c., to 1. The odds of course will be discriminated from the probabilities which are successively $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, &c. Thus on the first occasion on which a person sees a shark, and notices that it is accompanied by a little pilot fish, the odds are 1 to 1, or the probability $\frac{1}{2}$, that the next shark will be so accompanied.

When an event has happened a very great number of times, its happening once again approaches nearly to certainty. Thus if we suppose the sun to have risen demonstratively one thousand million times, the probability that it will rise again, on the ground of this knowledge merely, is $\frac{1{,}000{,}000{,}000+1}{1{,}000{,}000{,}000+1+1}$. But then the probability that it will continue to rise for as long a period as we know it to have risen is only $\frac{1{,}000{,}000{,}000+1}{2{,}000{,}000{,}000+1}$, or almost exactly $\frac{1}{2}$. The probability that it will continue so rising a thousand times as long is only about $\frac{1}{1001}$. The lesson which we may draw from these figures is quite that which we should adopt on other grounds, namely that experience never affords certain knowledge, and that it is exceedingly improbable that events will always happen as we observe them. Inferences pushed far beyond their data soon lose any considerable probability. De Morgan has said[d], 'No finite experience whatsoever can justify us in saying that the future shall coincide with the past in all time to come, or that there is any probability for such a conclusion.' On the other hand, we gain the assurance that experience sufficiently extended and prolonged will give us the knowledge of future events with an unlimited degree of probability, provided indeed that those events are not subject to arbitrary interference.

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[c] De Morgan's 'Essay on Probabilities,' Cabinet Cyclopædia, p. 67.

It must be clearly understood that these probabilities are only such as arise from the mere happening of the events, irrespective of any knowledge derived from other sources concerning those events or the general laws of nature. All our knowledge of nature is indeed founded in like manner upon observation, and is therefore only probable. The law of gravitation itself is only probably true. But when a number of different facts, observed under the most diverse circumstances, are found to be harmonized under a supposed law of nature, the probability of the law approximates closely to certainty. Each science rests upon so many observed facts, and derives so much support from analogies or direct connections with other sciences, that there are comparatively few cases where our judgment of the probability of an event depends entirely upon a few antecedent events, disconnected from the general body of physical science.

Events may often again exhibit a regularity of succession or preponderance of character, which the simple formula will not take into account. For instance, the majority of the elements recently discovered are metals, so that the probability of the next discovery being that of a metal, is doubtless greater than we calculated (p. 298). At the more distant parts of the planetary system, there are symptoms of disturbance which would prevent our placing much reliance on any inference from the prevailing order of the known planets to those undiscovered ones which may possibly exist at great distances. These and all like complications in no way invalidate the theoretic truth of the formulæ, but render their sound application much more difficult.

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[d] 'Treatise on Probability,' Cabinet Cyclopædia, p. 128.

Erroneous objections have been raised to the theory of probability, on the ground that we ought not to trust to our *à priori* conceptions of what is likely to happen, but should always endeavour to obtain precise experimental data to guide us[e]. This course, however, is perfectly in accordance with the theory, which is our best and only guide, whatever data we possess. We ought to be always applying the inverse method of probabilities so as to take into account all additional information. When we throw up a coin for the first time, we are probably quite ignorant whether it tends more to fall head or tail upwards, and we must therefore assume the probability of each event as $\frac{1}{2}$. But if it shows head, for instance, in the first throw, we now have very slight experimental evidence in favour of a tendency to show head. The chance of two heads is now slightly greater than $\frac{1}{4}$, which it appeared to be at first[f], and as we go on throwing the coin time after time, the probability of head appearing next time constantly varies in a slight degree according to the character of our previous experience. As Laplace remarks, we ought always to have regard to such considerations in common life. Events when closely scrutinized will hardly ever prove to be quite independent, and the slightest preponderance one way or the other is some evidence of connexion, and in the absence of better evidence should be taken into account.

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[e] J. S. Mill, 'System of Logic,' 5th Edition, bk. iii. chap. xviii. § 3.

[f] Todhunter's 'History,' pp. 472, 598.

The grand object of seeking to estimate the probability of future events from past experience, seems to have been entertained by James Bernouilli and De Moivre, at least such was the opinion of Condorcet; and Bernouilli may be said to have solved one case of the problem[g]. The English writers Bayes and Price are, however, undoubtedly the first who put forward any distinct rules on the subject[h]. Condorcet and several other eminent mathematicians advanced the mathematical theory of the subject; but it was reserved to the immortal Laplace to bring to the subject the full power of his genius, and carry the solution of the problem almost to perfection. It is instructive to observe that a theory which arose from the consideration of the most petty games of chance, the rules and the very names of which are in many cases forgotten, gradually advanced, until it embraced the most sublime problems of science, and finally undertook to measure the value and certainty of all our inductions.

Fortuitous Coincidences.

We should have studied the theory of probability to very little purpose, if we thought that it would furnish us with an infallible guide. The theory itself points out the possibility, or rather the approximate certainty, that we shall sometimes be deceived by extraordinary, but fortuitous coincidences. There is no run of luck so extreme that it may not happen, and it may happen to us, or in our time, as well as to other persons or in other times. We may be forced by all correct calculation to refer such coincidences to some necessary cause, and yet we may be deceived. All that the calculus of probability pretends to give, is *the result in the long run*, as it is called, and this really means in *an infinity of cases*. During any finite experience, however long, chances may be against us. Nevertheless the theory is the best guide we can have. If we always think and act according to its well interpreted indications, we shall have the best chance of escaping error; and if all persons, throughout all time to come, obey the theory in like manner, they will undoubtedly thereby reap the greatest advantage.

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[g] Todhunter's 'History,' pp. 378, 79.

[h] 'Philosophical Transactions' [1763], vol. liii. p. 370, and [1764], vol. liv. p. 296. Todhunter, pp. 294–300.

No rule can be given for descriminating between coincidences which are casual and those which are the effect of law or common conditions. By a fortuitous or casual coincidence, we mean an agreement between events, which nevertheless arise from wholly independent and different causes or conditions, and which will not always so agree. It is a fortuitous coincidence, if a penny thrown up repeatedly in various ways always falls on the same side; but it would not be fortuitous if there were any similarity in the motions of the hand, and the height of the throw, so as to cause or tend to cause a uniform result. Now among the infinitely numerous events, objects, or relations in the universe, it is quite likely that we shall occasionally notice casual coincidences. There are seven intervals in the octave, and there is nothing very improbable in the colours of the spectrum happening to be apparently divisible into the same or similar series of seven intervals. It is hardly yet decided whether this apparent coincidence, with which Newton was much struck, is well founded or not[i], but the question will probably be decided in the negative.

It is certainly a casual coincidence which the ancients noticed between the seven vowels, the seven strings of the lyre, the seven Pleiades, and the seven chiefs at Thebes[k].

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[i] 'Nature,' vol. i. p. 286.

[k] Aristotle's 'Metaphysics,' xiii. 6. 3.

principles of physical astronomy. Prout's Law bears more probability because it would bring the constitution of the elements themselves in close connexion with the atomic theory, representing them as built up out of a simpler substance.

In historical and social matters, coincidences are frequently pointed out which are due to chance, although there is always a strong popular tendency to regard them as the work of design, or as having some hidden cause. It has been pointed out that if to 1794, the number of the year in which Robespierre fell, we add the sum of its digits, the result is 1815, the year in which Napoleon fell; the repetition of the process gives 1830, the year in which Charles the Tenth abdicated. Again, the French Chamber of Deputies, in 1830, consisted of 402 members, of whom 221 formed the party called, 'La queue de Robespierre,' while the remainder, 181 in number, were named 'Les honnêtes gens.' If we give to each letter a numerical value corresponding to its place in the alphabet, it will be found that the sum of the values of the letters in each name exactly indicates the number of the party[m].

A number of such coincidences, often of a very curious character, might be adduced, and the probability against the occurrence of each may be enormously great. They must be attributed to chance, because they cannot be shown to have the slightest connexion with the general laws of nature; but persons are often found to be greatly influenced by such coincidences, regarding them as evidence of fatality, that is of a system of causation governing human affairs independently of the ordinary laws of nature. Let it be remembered that there are an infinite number of opportunities in life for some strange coincidence to present itself, so that it is quite to be expected that remarkable conjunctions will sometimes happen.

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[m] S. B. Gould's 'Curious Myths,' p. 222.

In all matters of judicial evidence, we must bear in mind the necessary occurrence from time to time of unaccountable coincidences. The Roman jurists refused for this reason to invalidate a testamentary deed, the witnesses of which had sealed it with the same seal. For witnesses independently using their own seals might be found to possess identical ones by accident[n]. It is well known that circumstantial evidence of apparently overwhelming completeness will sometimes lead to a mistaken judgment, and as absolute certainty is never really attainable, every court must act upon probabilities of a very high amount, and in a certain small proportion of cases they must almost of necessity condemn the innocent victims of a remarkable conjuncture of circumstances[o]. Popular judgments usually turn upon probabilities of far less amount, as when the palace of Nicomedia, and even the bedchamber of Diocletian, having been on fire twice within fifteen days, the people entirely refused to believe that it could be the result of accident. The Romans believed that there was a fatality connected with the name of Sextus.

> 'Semper sub Sextis perdita Roma fuit.'

The utmost precautions will not provide against all contingencies. To avoid errors in important calculations, it is usual to have them repeated by different computers, but a case is on record in which three computers made exactly the same calculations of the place of a star, and yet all did it wrong in precisely the same manner, for no apparent reason[p].

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[n] Possunt autem omnes testes et uno annulo signare testamentum. Quid enim si septem annuli una sculptura fuerint, secundum quod Pomponio visum est?—'Justinian,' ii. tit. x. 5.

[o] See Wills on 'Circumstantial Evidence,' p. 148.

[p] 'Memoirs of the Royal Astronomical Society,' vol. iv. p. 290, quoted by Lardner, 'Edinburgh Review,' July 1834, p. 278.

Summary of the Theory of Inductive Inference.

The theory of inductive inference adopted in this and the previous chapter, was chiefly suggested by the study of the Inverse Method of Probabilities, but it also bears much resemblance to the so-called Deductive Method described by Mr. J. S. Mill, in his well known 'System of Logic[q].' Mr. Mill's views concerning the Deductive Method, probably form the most original and valuable part of his treatise, and I should have ascribed the doctrine entirely to him, had I not found that the opinions put forward in other parts of his work are entirely inconsistent with the theory here upheld. As this subject is the most important and difficult one with which we have to deal, I will try to remedy the imperfect manner in which I have treated it, by giving a brief recapitulation of the views adopted.

All inductive reasoning is but an inverse application of deductive reasoning. Being in possession of certain particular facts or events expressed in propositions, we imagine some more general proposition expressing the existence of a law or cause; and, deducing the particular results of that supposed general proposition, we observe whether they agree with the facts in question. Hypothesis is thus always employed, consciously or unconsciously. The sole conditions to which we need conform in framing any hypothesis is, that we both have and exercise the power of inferring deductively from the hypothesis, to the particular logical combinations or results, which are to be compared with the known facts. Thus there are but three steps in the process of induction :—

(1) Framing of some hypothesis as to the character of the general law.

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[q] Book iii. chap. 11.

(2) Deducing consequences from that law.

(3) Observing whether the consequences agree with the particular facts under consideration.

In very simple cases of inverse reasoning, hypothesis may sometimes seem altogether needless. Thus, to take numbers again as a convenient illustration, I have only to look at the series,

1, 2, 4, 8, 16, 32, &c.,

to know at once that the general law is that of geometrical progression; I need no successive trial of various hypotheses, because I am familiar with the series, and have long since learnt from what general formula it proceeds. In the same way a mathematician becomes acquainted with the integrals of a number of common formulæ, so that we have no need to go through any process of discovery. But it is none the less true that whenever previous reasoning does not furnish the knowledge, hypotheses must be framed and tried. (See p. 142.)

There naturally arise two different cases, according as the nature of the subject admits of certain or only probable deductive reasoning. Certainty, indeed, is but a singular case of probability, and the general principles of procedure are always the same. Nevertheless, when certainty of inference is possible the process is simplified. Of several mutually inconsistent hypotheses, the results of which can be certainly compared with fact, but one hypothesis can ultimately be entertained. Thus in the inverse logical problem, two logically distinct conditions could not yield the same series of possible combinations. Accordingly in the case of two terms we had to choose one of seven different kinds of propositions, or in the case of three terms, our choice lay among 192 possible distinct hypotheses (pp. 154–164). Natural laws, however, are often quantitative in character, and the possible hypotheses are then infinite in variety.

When deduction is certain, comparison with fact is needed only to assure ourselves that we have rightly selected the hypothetical conditions. The law establishes itself, and no number of particular verifications can add to its probability. Having once deduced from the principles of algebra that the difference of the squares of two numbers is equal to the product of their sum and difference, no number of particular trials of its truth will render it more certain. On the other hand, no finite number of particular verifications of a supposed law will render that law certain. In short, certainty belongs only to the deductive process, and to the teachings of direct intuition; and as the conditions of nature are not given by intuition, we can only be certain that we have got a correct hypothesis when, out of a limited number conceivably possible, we select that one which alone agrees with the facts to be explained.

In geometry and kindred branches of mathematics, deductive reasoning is conspicuously certain, and it would often seem as if the consideration of a single diagram yields us certain knowledge of a general proposition. But in reality all this certainty is of a purely hypothetical character. Doubtless if we could ascertain that a supposed circle was a true and perfect circle, we could be certain concerning a multitude of its geometrical properties. But geometrical figures are physical objects, and the senses can never assure us as to their exact forms. The figures really treated in Euclid's 'Elements' are imaginary, and we never can verify in practice the conclusions which we draw with certainty in inference; questions of degree and probability enter.

Passing now to subjects in which deduction is only probable, it ceases to be possible to adopt one hypothesis to the exclusion of the others. We must entertain at the same time all conceivable hypotheses, and regard each with the degree of esteem proportionate to its probability. We go through the same steps as before.

(1) We frame an hypothesis.

(2) We deduce the probability of various series of possible consequences.

(3) We compare the consequences with the particular facts, and observe the probability that such facts would happen under the hypothesis.

The above processes must be performed for every conceivable hypothesis, and then the absolute probability of each will be yielded by the principle of the inverse method (p. 279). As in the case of certainty we accept that hypothesis which certainly gives the required results, so now we accept as most probable that hypothesis which most probably gives the results; but we are obliged to entertain at the same time all other hypotheses with degrees of probability proportionate to the probabilities that they would give the results.

So far we have treated only of the process by which we pass from special facts to general laws, that inverse application of deduction which constitutes induction. But the direct employment of deduction is often combined with the inverse. No sooner have we established a general law, than the mind rapidly draws other particular consequences from it. In geometry we may almost seem to infer that *because* one equilateral triangle is equiangular, therefore another is so. In reality it is not because one is that another is, but because all are. The geometrical conditions are perfectly general, and by what is sometimes called *parity of reasoning* whatever is true of one equilateral triangle, so far as it is equilateral, is true of all equilateral triangles.

Similarly, in all other cases of inductive inference, where we seem to pass from some particular instances to a new instance, we go through the same process. We form an hypothesis as to the logical conditions under which the given instances might occur; we calculate inversely the probability of that hypothesis, and compounding this with the probability that a new instance would proceed from the same conditions, we gain the absolute probability of occurrence of the new instance in virtue of this hypothesis. But as several, or many, or even an infinite number of mutually inconsistent hypotheses may be possible, we must repeat the calculation for each such conceivable hypothesis, and then the complete probability of the future instance will be the sum of the separate probabilities. The complication of this process is often very much reduced in practice, owing to the fact that one hypothesis may be nearly certainly true, and other hypotheses, though conceivable, may be so improbable as to be neglected without appreciable error. But when we possess no knowledge whatever of the conditions from which the events proceed, we may be unable to form any probable hypotheses as to their mode of origin. We have now to fall back upon the general solution of the problem effected by Laplace, which consists in admitting on an equal footing every conceivable ratio of favourable and unfavourable chances for the production of the event, and then accepting the aggregate result as the best which can be obtained. This solution is only to be accepted in the absence of all better means, but like other results of the calculus of probabilities, it comes to our aid where knowledge is at an end and ignorance begins, and it prevents us from over-estimating the knowledge we possess. The general results of the solution are in accordance with common sense, namely, that the more often an event has happened the more probable, as a general rule, is its subsequent occurrence. With the extension of experience this probability indefinitely increases, but at the same time the probability is slight that events will long continue to happen as they have previously happened.

We have now pursued the theory of inductive inference, as far as can be done with regard to simple logical or numerical relations. The laws of nature deal with time and space, which are indefinitely, or rather infinitely, divisible. As we passed from pure logic to numerical logic, so we must now pass from questions of discontinuous, to questions of continuous quantity, encountering fresh considerations of much difficulty. Before, therefore, we consider how the great inductions and generalizations of physical science illustrate the views of inductive reasoning just explained, we must break off for a time, and review the means which we possess of measuring and comparing magnitudes of time, space, mass, force, momentum, energy, and the various manifestations of energy in motion, heat, electricity, chemical change, and the other phenomena of nature.

BOOK III.

METHODS OF MEASUREMENT.

CHAPTER XIII.

THE EXACT MEASUREMENT OF PHENOMENA.

As physical science advances, it becomes more and more accurately quantitative. Questions of simple logical fact after a time resolve themselves into questions of degree, time, distance, or weight. Forces hardly suspected to exist by one generation, are clearly recognised by the next, and precisely measured by the third generation. But one condition of this rapid advance is the invention of suitable instruments of measurement. We need what Francis Bacon called *Instantiæ citantes*, or *evocantes*, methods of rendering minute phenomena perceptible to the senses; and we also require *Instantiæ radii* or *curriculi*, that is measuring instruments[a]. Accordingly, the introduction of a new instrument often forms an epoch in the history of science. As Davy said, 'Nothing tends so much to the advancement of knowledge as the application of a new instrument. The native intellectual powers of men in different times, are not so much the causes of the different success of their labours, as the peculiar nature of the means and artificial resources in their possession[b]'.

In the absence indeed of advanced theory and analytical power, a very precise instrument would be useless. Measuring apparatus and mathematical theory should advance *pari passu*, and with just such precision as the theorist can anticipate results, the experimentalist should be able to compare them with experience. The laborious and scrupulously accurate observations of Flamsteed, were the proper complement to the intense mathemetical powers of Newton.

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[a] 'Novum Organum,' bk. ii. Aphorisms 40, 45 and 46.

[b] 'Chemical Philosophy,' Works, vol. iv. p. 39. Quoted by Young, Works, vol. i. p. 576.

Every branch of knowledge commences with quantitative notions of a very rude character. After we have far progressed, it is often amusing to look back into the infancy of the science, and contrast present with past methods. At Greenwich Observatory in the present day, the hundredth part of a second is not thought an inconsiderable portion of time. The ancient Chaldæans recorded an eclipse to the nearest hour, and even the early Alexandrian astronomers thought it superfluous to distinguish between the edge and centre of the sun. By the introduction of the astrolabe, Ptolemy and the later Alexandrian astronomers could determine the places of the heavenly bodies within about ten minutes of arc. But little progress then ensued for thirteen centuries, until Tycho Brahe made the first great step towards accuracy, not only by employing better instruments, but even more by ceasing to regard an instrument as correct. Tycho, in fact, determined the errors of his instruments, and corrected his observations. He also took notice of the effects of atmospheric refraction, and succeeded in attaining an accuracy often sixty times as great as that of Ptolemy. Yet Tycho and Hevelius often erred several minutes in the determination of a star's place, and it was a great achievement of Rœmer and Flamsteed to reduce this error to seconds. Bradley, the modern Hipparchus, carried on the improvement, his errors in right ascension being under one second of time, and those of declination under four seconds of arc according to Bessel.

In the present day the average error of a single observation is probably reduced to the half or quarter of what it was in Bradley's time; and further extreme accuracy is attained by the multiplication of observations, and their skilful combination according to the theory of error.

Some of the more important constants, for instance that of nutation, have been determined within the tenth part of a second of space[c].

It would be a matter of great interest to trace out the dependence of this vast progress upon the introduction of new instruments. The astrolabe of Plotemy, the telescope of Galileo, the pendulum of Galileo and Huygens, the micrometer of Horrocks, and the telescopic sights and micrometer of Gascoygne and Picard, Rœmer's transit instrument, Newton's and Hadley's quadrant, Dollond's achromatic lenses, Harrison's chronometer, and Ramsden's dividing engine—such were some of the principal additions to astronomical apparatus. The result is, that we now take note of quantities, 300,000 or 400,000 times as small as in the time of the Chaldæans.

It would be interesting again to compare the scrupulous accuracy of a modern trigonometrical survey with Eratosthenes' rude but ingenious guess at the difference of latitude between Alexandria and Syene—or with Norwood's measurement of a degree of latitude in 1635. 'Sometimes I measured, sometimes I paced,' said Norwood; 'and I believe I am within a scantling of the truth.' Such was the germ of those elaborate geodesical measurements which have made the dimensions of the globe known to us within a few hundred yards.

In other branches of science, the invention of an instrument has usually marked, if it has not made, an epoch. The science of heat might be said to commence with the construction of the thermometer, and it has recently been advanced by the introduction of the thermo-electric pile. Chemistry has been created chiefly by the careful use of the balance, which forms a unique instance of an instrument remaining substantially in the form in which it was first applied to scientific purposes by Archimedes. The balance never has been and probably never can be improved, except in details of construction. On the other hand, the torsion balance, introduced by Coulomb towards the end of last century, has rapidly become essential in many branches of investigation. In the hands of Cavendish and Baily, it gave a determination of the earth's density; applied in the galvanometer, it gave a delicate measure of electrical forces, and was essential to the introduction of the thermo-electric pile. This balance is made by simply suspending any light rod by a thin wire or thread attached to the middle point. And we owe to it almost all the more delicate investigations in the theories of heat, electricity, and magnetism.

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[c] Baily, 'British Association Catalogue of Stars,' pp. 7, 23.

Though we can now take note of the millionth of an inch in space, and the millionth of a second in time, we must not overlook the fact that in other operations of science we are yet in the position of the Chaldæans. Not many years have elapsed since the magnitudes of the stars, meaning the amount of light they send to the observer's eye, were guessed at in the rudest manner, and the astronomer adjudged a star to this or that order of magnitude by a rough comparison with other stars of the same order. To the late Sir John Herschel we owe an attempt to introduce an uniform method of measurement and expression, bearing some relation to the real photometric magnitudes of the stars[d]. Previous to the researches of Bunsen and Roscoe on the chemical action of light, we were absolutely devoid of any mode of measuring the energy of light; even now the methods are tedious, and it is not clear that they give the energy of light so much as one of its special effects. Many natural phenomena have hardly yet been made the subject of measurement at all, such as the intensity of sound, the phenomena of taste and smell, the magnitude of atoms, the temperature of the electric spark or of the sun's photosphere.

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[d] 'Outlines of Astronomy,' 4th ed. sect. 781, p. 522. 'Results of Observations at the Cape of Good Hope,' &c., p. 371.

To suppose, then, that quantitative science treats only of exactly measurable quantities, is a gross if it be a common mistake. Whenever we are treating of an event which either happens altogether or does not happen at all, we are engaged with a non-quantitative phenomenon, a matter of fact, not of degree; but whenever a thing may be greater or less, or twice or thrice as great as another, whenever, in short, ratio enters even in the rudest manner, there science will have a quantitative character. There can be little doubt, indeed, that every science as it progresses will become gradually more and more quantitative. Numerical precision is doubtless the very soul of science, as Herschel said[e], and as all natural objects exist in space, and involve molecular movements, measurable in velocity and extent, there is no apparent limit to the ultimate extension of quantitative science. But the reader must not for a moment suppose that, because we depend more and more upon mathematical methods, we leave logical methods behind us. Number, as I have endeavoured to show, is logical in its origin, and quantity is but a development of number, or is analogous thereto.

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[e] 'Preliminary Discourse on the Study of Natural Philosophy,' p. 122.

Division of the Subject.

The general subject of quantitative investigation will have to be divided into several parts. We shall, firstly, consider the means at our disposal for measuring phenomena, and thus rendering them more or less amenable to mathematical treatment. This task will involve an analysis of the principles on which accurate methods of measurement are founded, forming the subject of the remainder of the present chapter. As measurement, however, only yields ratios, we have in the next chapter (XIV) to consider the establishment of unitary magnitudes, in terms of which our results may be expressed. As every phenomenon is usually the sum of several distinct quantities proceeding from different causes, we have next to investigate in Chapter XV the methods by which we may disentangle complicated effects, and refer each part of the joint effect to its separate cause.

It yet remains for us in subsequent chapters to treat of quantitative induction, properly so called. We must follow out the inverse logical method, as it presents itself in problems of a far higher degree of difficulty than those which treat of objects related in a simple logical manner, and incapable of merging into each other by addition and subtraction.

Continuous Quantity.

The phenomena of nature are for the most part manifested in quantities which increase or decrease continuously. When we inquire into the precise meaning of continuous quantity, we find that it can only be described as that which is divisible without limit. We can divide a millemetre into ten, or one hundred, or one thousand, or ten thousand parts, and mentally at any rate we can carry on the process *ad infinitum*. Any finite space, then, must be conceived as made up of an infinite number of parts, each of which must consequently be infinitely small. We cannot entertain some of the simplest geometrical notions without allowing this. The conception of a square involves the conception of a side and diagonal, which, as Euclid admirably proves in the 117th proposition of his tenth book, have no common measure[f], meaning, as I apprehend, no finite common measure. Incommensurable quantities are, in fact, those which have for their only common measure an infinitely small quantity. It is somewhat startling to find, too, that in theory incommensurable quantities will be infinitely more frequent than commensurable. Let any two lines be drawn haphazard; it is infinitely unlikely that they will be commensurable, so that the commensurable quantities, which we are supposed to deal with in practice, are but singular cases among an infinitely greater number of incommensurable cases.

Practically, however, we treat all quantities as made up of the least quantities which our senses, assisted by the best measuring instruments, can appreciate. So long as microscopes were uninvented, it was sufficient to regard an inch as made up of a thousand thousandths of an inch; now we must treat it as composed of a million millionths. We might apparently avoid all mention of infinitely small quantities, by never carrying our approximations beyond quantities, which the senses can appreciate. In geometry, as thus treated, we should never assert two quantities to be equal, but only to be *apparently* equal. Legendre really adopts this mode of treatment in the twentieth proposition of the first book of his Geometry; and it is practically adopted throughout the physical sciences, as we shall afterwards see. But though our fingers, and senses, and instruments must stop somewhere, there is no reason why the mind should not go on. We can see that a proof which is only carried through a few steps, in fact, might be carried on without limit, and it is this consciousness of no stopping place, which renders Euclid's proof of his 117th proposition so impressive. Try how we will to circumvent the matter, we cannot really avoid the consideration of the infinitely small and the infinitely great. The same methods of approximation which seem confined to the finite, mentally extend themselves to the infinite[g].

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[f] See De Morgan, 'Study of Mathematics,' in U. K. S. Library, p. 81.

One result which immediately follows from these considerations is, that we cannot possibly adjust any two quantities in absolute equality. The suspension of Mahomet's coffin between two precisely equal magnets, is theoretically conceivable but practically impossible. The story of the 'Merchant of Venice,' turns upon the infinite improbability, that an exact quantity of flesh could be cut. Unstable equilibrium cannot exist in nature, for it is that which is destroyed by an infinitely small displacement. It might be possible to balance an egg on its end practically, because no egg has a surface of perfect curvature. Suppose the egg shell to be perfectly smooth, and the feat would become impossible.

The Fallacious Indications of the Senses.

I may briefly remind the reader how little we can trust to our unassisted senses in estimating the degree, quantity, or magnitude of any phenomenon. The eye cannot correctly estimate the comparative brightness of two luminous bodies which differ much in brilliancy; for we know that the iris is constantly adjusting itself to the intensity of the light received, and thus admits more or less light according to circumstances. The moon which shines with almost dazzling brightness by night, is pale and nearly imperceptible while the eye is yet affected by the vastly more. powerful light of day. Much has been recorded concerning the comparative brightness of the zodiacal light at different times [h], but it would be difficult to prove that these changes are not due to the varying darkness at the time, or the different acuteness of the observer's eye. For a like reason it is exceedingly difficult to establish the existence of any change in the form or comparative brightness of nebulæ; the appearance of a nebula greatly depends upon the keenness of sight of the observer, or the accidental condition of freshness or fatigue of his eye; the same is true of lunar observations [i]; and even the use of the best telescope fails to remedy this difficulty. In judging of colours again, we must remember that light of any given colour tends to dull the sensibility of the eye for light of the same colour.

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[g] Lacroix, 'Essai sur l'Enseignement ou manière d'étudier les Mathématiques,' 2nd ed. Paris, 1816, pp. 292–294.

Nor is the eye when unassisted by instruments a much better judge of magnitude. Our estimates of the size of minute bright points, such as the fixed stars, are completely falsified by the effects of irradiation. Tycho calculated from the apparent size of the star-discs, that no one of the principal fixed stars could be contained within the area of the earth's orbit. Apart, however, from irradiation or other distinct causes of error, our visual estimates of sizes and shapes are often astonishingly incorrect. Artists almost invariably draw distant mountains or other objects in ludicrous disproportion to nearer objects, as a comparison of a sketch with a photograph at once shows. The extraordinary apparent difference of size of the sun or moon, according as it is high in the heavens or near the horizon, should be sufficient to make us cautious in accepting the plainest indications of our senses, unassisted by instrumental measurement. As to statements concerning the height of the aurora and the distance of meteors, they are to be utterly distrusted. When Captain Parry says that a ray of the aurora shot suddenly downwards between him and the land which was only 3000 yards distant, we must consider him subject to an error of sense[l].

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[h] 'Cosmos.' Translated by Otté, vol. i. pp. 131–134.

[i] 'Report of the British Association,' 1871, p. 84. Grant's 'History of Physical Astronomy,' pp. 568–9.

It is true that errors of observation are more usually errors of judgment than of sense. That which is actually seen must be truly seen so far; and if we correctly interpret the meaning of the phenomenon, there would be no error at all. But the weakness of the bare senses as measuring instruments, arises from the fact that they import varying conditions of unknown amount, and we cannot make the requisite corrections and allowances as in the case of a solid and invariable instrument.

Bacon has excellently stated the insufficiency of the senses for estimating the magnitudes of objects, or detecting the degrees in which phenomena present themselves. 'Things escape the senses,' he says[m], 'because the object is not sufficient in quantity to strike the sense: as all minute bodies; because the percussion of the object is too great to be endured by the senses: as the form of the sun when looking directly at it in mid-day; because the time is not proportionate to actuate the sense: as the motion of a bullet in the air, or the quick circular motion of a firebrand, which are too fast, or the hour-hand of a common clock, which is too slow; from the distance of the object as to place: as the size of the celestial bodies, and the size and nature of all distant bodies;

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[l] Loomis, 'On the Aurora Borealis.' Smithsonian Transactions, quoting Parry's Third Voyage, p. 61.

[m] 'Novum Organum.'

from prepossession by another object: as one powerful smell renders other smells in the same room imperceptible; from the interruption of interposing bodies: as the internal parts of animals; and because the object is unfit to make an impression upon the sense: as the air or the invisible and untangible spirit which is included in every living body.'

Complexity of Quantitative Questions.

One remark which we may well make in entering upon quantitative questions, has regard to the great variety and extent of phenomena presented to our notice. So long as we deal only with a simply logical question, that question is merely, Does a certain event happen? or, Does a certain object exist? No sooner do we regard the event or object as capable of more or less, than one question branches out into many. We must now ask, How much is it compared with its cause or necessary condition? Does it change when the amount of the cause changes? If so, does it change in the same or opposite direction? Is the change in simple proportion to that of the cause? If not, what more complex law of connection holds true? This law determined satisfactorily in one series of circumstances may be varied under new conditions, and the most complex relations of several quantities may ultimately be established.

In every question of physical science there is thus a series of steps of progress, the first one or two of which are usually made with ease, while the succeeding ones demand more and more careful measurement. We cannot lay down any single invariable series of questions which must be asked from nature. The exact character of the questions will vary according to the nature of the case, but they will usually be of a very evident kind, and we may readily illustrate them by actual examples. Suppose, for instance, that we are investigating the solution of some salt in water. The first is a purely logical question: Is there solution, or is there not? Assuming the answer to be in the affirmative, we next inquire, Does the solubility vary with the temperature, or not? In all probability some variation will be found to exist, and we shall have at the same time an answer to the further question, Does the quantity dissolved increase, or does it diminish with the temperature? In by far the greatest number of cases salts and substances of all kinds dissolve more freely the higher the temperature of the water, but there are a few salts, such as calcium sulphate, which follow the opposite rule. A considerable number of salts resemble sodium sulphate in becoming more soluble up to a certain temperature, and then varying in the opposite direction. We next require to assign the amount of variation as compared with that of the temperature, assuming at first that the increase of solubility is proportional to the increase of temperature. Common salt is an instance of very slight variation, and potassium nitrate of very considerable increase with temperature. Very accurate observations will probably show, however, that the simple law of proportionate variation is only approximately true, and some more complicated law involving the second, third, or higher powers of the temperature may ultimately be established. All these investigations have to be carried out for each salt separately, since no distinct principles by which we may infer from one substance to another have yet been detected. There is still an indefinite field for further research open; for the solubility of salts would probably vary with the pressure under which the medium is placed; the presence of other salts already dissolved may have effects yet unknown. The researches already effected as regards the solvent power of water must be repeated as regards alcohol, ether, carbon bisulphide, and other media, so that unless general laws can be detected, this one phenomenon of solution can never be exhaustively treated. The same kind of questions recur as regards the solution or absorption of gases in liquids, the pressure as well as the temperature having then a most decided effect, and Professor Roscoe's researches on the subject present an excellent example of the successive determination of various complicated laws[n].

There is hardly a single branch of scientific research in which similar complications are not ultimately encountered. In the case of gravity, indeed, we arrive at the final law, that the force is invariably the same for all kinds of matter, and depends only on the distance of action. But in other subjects the laws, if simple in their ultimate nature, are disguised and complicated in their apparent results. Thus the effect of heat in expanding solids, or the reverse effect of forcible extension or compression upon the temperature of a body, will vary from one substance to another, will vary as the temperature is already higher or lower, and will probably follow a highly complex law, which in some cases gives negative or exceptional results. In crystalline substances the same researches have to be repeated in each distinct axial direction.

In the sciences of pure observation again, such as those of astronomy, meteorology, and terrestrial magnetism, we meet with many interesting series of quantitative determinations. The so-called fixed stars, as Giordano Bruno divined, are not really fixed, and may be more truly described as vast wandering orbs, each pursuing its own path through space. We must then determine separately for each star the following questions :—

1. Does it move ?
2. In what direction ?

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[n] Watt's 'Dictionary of Chemistry,' vol. ii. p. 79?.

3. At what velocity?
4. Is this velocity variable or uniform?
5. If variable, according to what law?
6. Is the direction uniform?
7. If not, what is the form of the apparent path?

The successive answers to such questions in the case of certain binary stars, have afforded a proof that the motions are due to a central force coinciding in law with gravity, and doubtless identical with it. In other cases the motions are usually so small that it is exceedingly difficult to distinguish them with certainty. A coincidence of motions in some constellations has been pointed out by Mr. Proctor, and the parallactic effect due to the sun's proper motion has been surely detected; but the time is yet far off when any general results as regards stellar motions can be established.

The variation in the brightness of stars opens an unlimited field for curious observation. There is not a star in the heavens concerning which we might not have to determine—

1. Does it vary in brightness?
2. Is the brightness increasing or decreasing?
3. Is the variation uniform, that is, simply proportional to time?
4. If not, according to what law does it vary?

In a majority of cases the change will probably be found to have a periodic character, in which case several other questions will arise, such as—

5. What is the length of the period?
6. Are there minor periods within the principal period?
7. What is the form or law of variation within the period?
8. Is there any change in the amount of variation?

9. If so, is it a secular, i. e. a continually growing change, or does it give evidence of a greater period?

Already the periodic changes of a certain number of stars have been determined with accuracy, and the lengths of the periods vary from less than three days up to intervals of time at least 250 times as great. Periods within periods have also been detected[o].

There is, perhaps, no subject in which more complicated quantitative conditions have to be determined than terrestrial magnetism. Since the time when the declination of the compass was first noticed, as some suppose by Columbus, we have had successive discoveries from time to time of the progressive change of declination from century to century; of the periodic character of this change; of the difference of the declination in various parts of the earth's surface; of the varying laws of the change of declination; of the dip or inclination of the needle, and the corresponding laws of its periodic changes; the horizontal and perpendicular intensities have also been the subject of exact measurement, and have been found to vary by place and time, like the directions of the needle; daily and yearly periodic changes have also been detected, and all the elements are found to be subject to occasional storms or abnormal perturbations, in which the eleven year period, now known to be common to many planetary relations, is apparent. The complete solution of these motions of the compass needle involves nothing less than a determination of its position and oscillations in every part of the world at any epoch, the like determination for another epoch, and so on, time after time, until the periods of all changes are ascertained, and the character of the variations determined. This one subject offers to men of science an almost inexhaustible field for interesting quantitative research[p], in which we shall doubtless at some future time discover the operation of causes now most mysterious and unaccountable.

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[o] Humboldt's 'Cosmos,' translated by Otté, vol. iii. p. 228.

The Methods of Accurate Measurement.

In studying the modes by which physicists have accomplished very exact measurements, we find that they are very various, but that they may perhaps be reduced under the following three classes :—

1. The increase or decrease of the quantity to be measured in some determinate ratio, so as to bring it within the scope of our senses, and to equate it with the standard unit, or some determinate multiple or sub-multiple of this unit.

2. The discovery of some natural conjunction of events which will enable us to compare directly the multiples of the quantity with those of the unit, or a quantity related in a definite ratio to that unit.

3. Indirect measurement, which gives us not the quantity itself, but some other quantity connected with it by known mathematical relations.

Conditions of Accurate Measurement.

Several conditions are requisite in order that a measurement may be made with great accuracy, and that the result may be closely accordant when several independent measurements are made.

In the first place the magnitude must be exactly defined by sharp terminations, or precise marks of inconsiderable thickness. When a boundary is vague and graduated, like the penumbra in a lunar eclipse, it is impossible to say where the end really is, and different people will come to different results. We may sometimes overcome this difficulty to a certain extent, by observations repeated in a special manner, as we shall afterwards see; but when possible, we should choose opportunities for measurement when precise definition is easy. The moment of occultation of a star by the moon can be observed with great accuracy, because the star disappears with perfect suddenness; but there are many other astronomical conjunctions, eclipses, transits, &c., which occupy a certain length of time in happening, and thus open the way to differences of opinion. It would be impossible to observe with precision the movements of a body possessing no definite points of reference. The spots on the sun, for instance, furnish the only direct criterion of its rotation, and the possibility that these spots have a tendency to move in one direction throws a doubt upon all determinations of the sun's axial movement.

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[p] Gauss, 'General Theory of Terrestrial Magnetism'; Taylor's 'Scientific Memoirs,' vol. ii. p. 228.

The colours of the complete spectrum shade with perfect continuity into each other, so that their separation is entirely an arbitrary matter. Exact determinations of refractive indices would have been impossible, had we not the fixed dark lines of the solar spectrum as precise points for measurement, or, what comes to the same thing, various kinds of homogeneous light, such as that of sodium, possessing a nearly uniform length of vibration.

In the second place, we cannot measure accurately unless we have the means either of multiplying or dividing a quantity without considerable error, so that we may correctly equate one magnitude with the multiple or submultiple of the other. In some cases we operate upon the quantity to be measured, and bring it into accurate coincidence with the actual standard, as when in photometry we vary the distance of our luminous body, until its illuminating power at a certain point is equal to that of a standard lamp. In other cases we repeat the unit until it equals the object, as in surveying land, or determining a weight by the balance. The requisites of accuracy now are:—(1) That we can repeat unit after unit of exactly equal magnitude; (2) That these can be joined together so that the aggregate shall really be the sum of the parts. The same conditions apply to subdivision, which may be regarded as a multiplication of subordinate units. In order to measure to the thousandth of an inch, we must be able to add thousandth after thousandth without error in the magnitude of these spaces, or in their conjunction.

The condenser electrometer, as remarked by Thomson and Tait[q], is a good example of an instrument unfitted to give any sure measure of electro-motive force, because the friction between the parts of the condenser often produces more electricity than the original quantity which was to be measured.

Measuring Instruments.

To consider the mechanical construction of scientific instruments, is no part of my purpose in this book. I wish to point out merely the general purpose of such instruments, and the methods adopted to carry out that purpose with great precision. In the first place we must distinguish between the instrument which effects a comparison between two quantities, and the standard magnitude which often forms one of the quantities compared. The astronomer's clock, for instance, is no standard of the efflux of time; it serves but to subdivide, with approximate accuracy, the interval of successive passages of a star across the meridian, which it may effect perhaps to the tenth part of a second, or $\frac{1}{864000}$ part of the whole.

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[q] 'Elements of Natural Philosophy,' sect. 326, p. 108.

The moving globe itself is the real standard clock, and the transit instrument the finger of the clock, while the stars are the hour, minute, and second marks, none the less useful or accurate because they are disposed at unequal intervals. The photometer is a simple instrument, by which we compare the relative intensity of rays of light falling upon a given spot. The galvanometer shows the comparative intensity of electric currents passing through a wire. The calorimeter guages the quantity of heat passing from a given object. But no such instruments furnish the standard unit in terms of which our results are to be expressed. In one peculiar case alone does the same instrument combine the unit of measurement and the means of comparison. A theodolite, mural circle, sextant, or other instrument for the measurement of angular magnitudes has no need of an additional physical unit; for the very circle itself, or complete revolution, is the natural unit to which all greater or lesser amounts of angular magnitude are referred.

The result of every measurement is to make known the purely numerical ratio existing between the magnitude to be measured, and a certain other magnitude, which should, when possible, be a fixed unit or standard magnitude, or at least an intermediate unit of which the value can be ascertained in terms of the ultimate standard. But though a ratio is the required result, an equation is the mode in which the ratio is determined and expressed. In every measurement we equate some multiple or submultiple of one quantity, with some multiple or submultiple of another, and equality is always the fact which we ascertain by the senses. By the eye, the ear, or the touch, we judge whether there is a discrepancy or not between two lights, two sounds, two intervals of time, two bars of metal. Often indeed we substitute one sense for the other, as when the efflux of time is judged by the marks upon the standard unit until we get a magnitude equal to that to be measured. Ordinary measurement by a foot rule, a surveyor's chain, or the excessively careful measurements of the base line of a trigonometrical survey by standard bars form a sufficient instance of this case.

In the second case, where $p\frac{x}{y}=q$, we multiply or divide a magnitude until we get what is equal to the unit, or to some magnitude easily comparable with it. As a general rule the quantities which we desire to measure in physical science are too small rather than too great for easy determination, and the problem consists in multiplying them without introducing error. Thus the expansion of a metallic bar when heated from 0° C to 100° may be multiplied by a train of levers or cog wheels. In the common thermometer the expansion of the mercury is rendered very apparent, and easily measurable by the fineness of the tube, and many other cases might be quoted. There are some phenomena, on the contrary, which are too great or rapid to come within the easy range of our senses, and our task is then the opposite one of diminution. Galileo found it difficult to measure the velocity of a falling body, owing to the very considerable velocity acquired in a single second. He adopted the elegant device, therefore, of lessening the rapidity by letting the body roll down an inclined plane, which enables us to reduce the accelerating force in any required ratio. The same purpose is effected in the well known experiments performed on Attwood's machine, and the measurement of gravity by the pendulum really depends on the same principle applied in a far more advantageous manner. Sir Charles Wheatstone has invented a beautiful method of galvanometry for strong currents, which consists in drawing off from the main current a certain determinate portion, which is equated by the galvanometer to a standard current[r]. In short, he measures not the current itself but a known fraction of it.

In many electrical and other experiments, we wish to measure the movements of a needle or other body, which are not only very slight in themselves, but the manifestations of exceedingly small forces. We cannot even approach a delicately balanced needle without disturbing it. Under these circumstances the only mode of proceeding with accuracy, is to attach a very small mirror to the moving body, and employ a ray of light reflected from the mirror as an index of its movements. The ray may be considered quite incapable of affecting the body, and yet by allowing the ray to pass to a sufficient distance, the motions of the mirror may be increased to almost any extent. A ray of light is in fact a perfectly weightless finger or index of indefinite length, with the additional advantage that the angular deviation is by the law of reflection double that of the mirror. This method, was introduced by Gauss, and is now of great importance; but in Wollaston's reflecting goniometer a ray of light had previously been employed as an index finger. Lavoisier and Laplace had also used a telescope in connection with the pyrometer.

It is a great advantage in some instruments that they can be readily made to manifest a phenomenon in a greater or less degree, by a very slight change in the construction. Thus either by enlarging the bulb or contracting the tube of the thermometer, we can make it give more conspicuous indications of change of temperature. The barometer, on the other hand, always gives the variations of pressure on one scale. The torsion balance is especially remarkable for the extreme delicacy which may be attained by increasing the length and lightness of the rod, and the length and thinness of the supporting thread. Forces so minute as the attraction of gravitation between two balls, or the magnetic and diamagnetic attraction of common liquids and gases, may thus be made apparent, and even measured. The common chemical balance, too, is capable theoretically of indefinite sensibility[s].

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[r] De la Rive's 'Electricity,' vol. ii. pp. 897, 98.

The third mode of measurement, which may be called the Method of Repetition, is of such great importance and interest that we must consider it in a separate section. It consists in multiplying both magnitudes to be compared until some multiple of the first is found to coincide very nearly with some multiple of the second. If the multiplication can be effected to an indefinite extent, without the introduction of countervailing errors, the accuracy with which the required ratio can be determined is unlimited, and we thus account for the extraordinary precision with which intervals of time in astronomy are compared together.

The fourth mode of measurement in which we equate submultiples of two magnitudes is comparatively seldom employed, because it does not conduce to accuracy. In the photometer, perhaps, we may be said to use it; we compare the intensity of two sources of light, by placing them both at such distances from a given surface, that the light falling on the surface is tolerable to the eye, and equally intense from each source. Since the intensity of rays diminishes, as the inverse squares of the distances, the relative intensities of the luminous bodies are proportional to the squares of their distances. The equality of intensity of two rays of similarly coloured light, may be most accurately ascertained in the mode suggested by Arago, namely, by causing the rays to pass in opposite directions through two nearly flat lenses pressed together.

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[s] Watt's 'Dictionary of Chemistry,' art. *Balance*, vol. i. p. 487.

There is an exact equation between the intensities of the beams when Newton's rings disappear, the ring created by one ray being exactly the complement of that created by the other[t].

The Method of Repetition.

The ratio of two quantities can be determined with unlimited accuracy, if we can multiply both the object of measurement and the standard unit without error, and then observe what multiple of the one coincides or nearly coincides with some multiple of the other. Although perfect coincidence can never be really attained, the error thus arising may be indefinitely reduced. For if the equation $py = qx$ be uncertain to the amount e, so that $py = qx \pm e$, then we have $p = q\frac{x}{y} \pm \frac{e}{y}$, and as we are supposed to be able to make y as great as we like without increasing the error e, it follows that we can approximate as closely as we like to the required ratio $x \div y$.

This method of repetition is naturally employed whenever quantities can be repeated, or repeat themselves without error of juxtaposition, which is especially the case with the motions of the earth and heavenly bodies. In determining the length of the sidereal day, we really determine the ratio between the earth's revolution round the sun, and its rotation on its own axis. We might ascertain the ratio by observing the successive passages of a star across the zenith, and comparing the interval by a good clock with that between two passages of the sun, the difference being due to the angular movement of the earth round the sun. In such observations we should have an error of a considerable part of a second at each observation, in addition to the irregularities of the clock. But the revolutions of the earth repeat themselves day after day, and year after year, without the slightest interval between the end of one period and the beginning of another. The operation of multiplication is perfectly performed for us by nature. If, then, we can find an observation of the passage of a star across the meridian a hundred years ago, that is of the interval of time between the passage of the sun and the star, the instrumental errors in measuring this interval by a clock and telescope may be greater than in the present day, but will be divided by about 36,524 days, and rendered excessively small. It is thus that astronomers have been able to ascertain the ratio of the mean solar to the sideral day to the 8th place of decimals (1·00273791 to 1), or to the hundred millionth part, probably the most accurate result of measurement in the whole range of science.

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[t] Humboldt's 'Cosmos,' (Bohn), vol. iii. p. 129.

The antiquity of this mode of comparison is almost as great as that of astronomy itself. Hipparchus made the first clear application of it, when he compared his own observations with those of Aristarchus, made 145 years previously [u], and thus ascertained the length of the year. This calculation may in fact be regarded as the earliest attempt at an exact determination of the constants of nature. The method is the main resource of astronomers; Tycho, for instance, detected the slow diminution of the obliquity of the earth's axis, by the comparison of observations at long intervals. Living astronomers use the method as much as earlier ones; but so superior in accuracy are all observations taken during the last hundred years to all previous ones, that it is often found preferable to take a shorter interval, rather than incur the risk of greater instrumental errors in the earlier observations.

[u] Montucla, 'Histoire des Mathématiques,' vol. i. p. 258.

It is obvious that many of the slower changes of the heavenly bodies must require the lapse of large intervals of time to render their amount perceptible. Hipparchus could not possibly have discovered many of the smaller inequalities of the heavenly motions, because there were no previous observations of sufficient age or exactness to exhibit them. And just as the observations of Hipparchus formed the starting-point for subsequent comparisons, so a large part of the labour of present astronomers is directed to recording the present state of the heavens so exactly, that future generations of astronomers may detect many changes, which cannot possibly become known in the present age.

The principle of repetition was very ingeniously employed in an instrument first proposed by Mayer in 1767, and carried into practice in the Repeating Circle of Borda[v]. The exact measurement of angles is indispensable, not only in astronomy but also in trigonometrical surveys, and the highest skill in the mechanical execution of the graduated circle and telescope will not prevent terminal errors of considerable amount. If instead of one telescope, the circle be provided with two similar telescopes, these may be alternately directed to two distant points, say the marks in a trigonometrical survey, so that the circle shall be turned through any multiple of the angle subtended by those marks, before the amount of the angular revolution is read off upon the graduated circle. Theoretically speaking, all error arising from imperfect graduation might thus be indefinitely reduced, being divided by the number of repetitions. In practice, however, the advantage of the invention is not found to be great, probably because a certain error is introduced at each observation in the changing or fixing of the telescopes. It is moreover inapplicable to moving objects like the heavenly bodies, so that its use is confined to important trigonometrical surveys.

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[v] Young, 'Works,' vol. ii. p. 546.

The pendulum is the most perfect of all instruments, chiefly because it naturally admits of almost indefinite repetition. Since the force of gravity never ceases, one swing of the pendulum is no sooner ended than the other is begun, so that the juxtaposition of successive units is absolutely perfect. Provided that the oscillations be equal, then one thousand oscillations will occupy exactly one thousand times as great an interval of time as one oscillation. Not only is the subdivision of time entirely dependent on this fact, but in the accurate measurement of gravity, and many important determination it is of the greatest service. In the deepest mine, we could not observe the rapidity of fall of a body for more than a quarter of a minute, and the measurement of its velocity would be difficult, and subject to uncertain errors from resistance of air, &c. In the pendulum, we have a body which can be kept rising and falling for many hours, in a medium entirely under our command or if desirable in a vacuum. Moreover, the comparative force of gravity at different points, at the top and bottom of a mine for instance, can be determined with wonderful precision, by comparing the oscillations of two exactly similar pendulums, with the aid of electric clock signals. To ascertain the comparative lengths of vibration of two pendulums, it is only requisite to swing them one in front of the other, to record by a clock the moment when they coincide in swing, so that one hides the other, and then count the number of vibrations until they again come to similar coincidence. If one pendulum makes m vibrations and the other n, we at once have our equation $pn = qm$; which gives the length of vibration of either pendulum in terms of the other. This method of coincidence, embodying the principle of repetition in perfection, was employed with wonderful skill by Sir George Airy, in his experiments on the Density of the Earth at the Harton Colliery; the pendulums above and below being compared with clocks, which again were compared with each other by electric signals. So exceedingly accurate was this method of observation, as carried out by Sir George Airy, that he was able to measure a total difference in the vibrations at the top and bottom of the shaft, amounting to only 2·24 seconds in the twenty-four hours, with an error of less than one hundredth part of a second, or one part in 8,640,000 of the whole day [x].

The principle of repetition has been elegantly applied in observing the motion of waves in water. If the canal in which the experiments are made be short, say twenty feet long, the waves will pass through it so rapidly that an observation of one length, as practised by Walker, will be subject to much terminal error, even when the observer is very skilful. But it is a result of the undulatory theory that a wave is quite unaltered, and loses no time by complete reflection, so that it may be allowed to travel backwards and forwards in the same canal, and its motion, say through sixty lengths, or 1200 feet, may be observed with the same accuracy as in a canal 1200 feet long, with the advantage of greater uniformity in the condition of the canal and water [y]. It is always desirable, if possible, to bring an experiment into a small compass, so as to be well under command, and yet we may often by repetition enjoy at the same time the advantage of extensive observation.

One reason of the great accuracy of weighing with a good balance is the fact, that weights placed in the same scale are naturally added together without the slightest error. There is no difficulty in the precise juxtaposition of two grammes, but the juxtaposition of two metre measures can only be effected with tolerable accuracy, by the use of microscopes and many precautions. Hence, the extreme trouble and cost attaching to the exact measurement of a base line for a survey, the risk of error entering at every juxtaposition of the measuring bars, and indefatigable attention to all the requisite precautions being necessary throughout the operation[z].

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[x] 'Philosophical Transactions,' (1856) vol. 146, Part i. p. 297.

[y] Airy, 'On Tides and Waves,' Encyclopædia Metropolitana, p. 345. Scott Russell, 'British Association Report,' 1837, p. 432.

Measurements by Natural Coincidence.

In certain cases a peculiar conjunction of circumstances enables us to dispense more or less with instrumental aids, and to obtain the most exact numerical results in the simplest manner. The mere fact, for instance, that no human being has ever seen a different face of the moon from that familiar to us, conclusively proves that the period of rotation of the moon on its own axis is equal to that of its revolution round the earth. Not only have we the repetition of these movements during 1000 or 2000 years at least, but we have observations made for us at very remote periods, free from instrumental error, no instrument being needed. We learn that the seventh satellite of Saturn is subject to a similar law, because its light undergoes a variation in each revolution, owing to the existence of some dark tract of land; now this failure of light always occurs while it is in the same position relative to Saturn, clearly proving the equality of the axial and revolutional periods, as Huyghens perceived[a].

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[z] Herschel's, 'Familiar Lectures on Scientific Subjects,' p. 184.

[a] 'Hugenii Cosmotheoros,' pp. 117–18. Laplace's 'Système,' translated, vol. i. p. 67.

modes. When the moon is exactly half full, the moon, sun, and earth, are at the angles of a right-angled triangle. He proposed therefore at such a time to measure the moon's elongation from the sun, which would give him the two other angles of the triangle, and enable him to judge of the comparative distances of the moon and sun from the earth. His result, though very rude, was far more accurate than any notions previously entertained, and enabled him to form some estimate of the comparative magnitudes of the bodies. Eclipses of the moon were also very useful in ascertaining the longitudes of the stars, which were invisible when the sun was above the horizon. For the moon when eclipsed must be 180° distant from the sun; hence it was only requisite to measure the distance of a fixed star in longitude from the eclipsed moon to obtain with ease its angular distance from the sun.

In later times the eclipses of Jupiter have usefully served to give a measure of an angle; for at the middle moment of the eclipse the satellite must be exactly in the same straight line with the planet and sun, so that we can learn from the known laws of movement of the satellite the longitude of Jupiter as seen from the sun. If at the same time we measure the elongation or apparent angular distance of Jupiter from the sun, as seen from the earth, we have all the angles of the triangle between Jupiter, the sun, and the earth, and can calculate the comparative magnitudes of the sides of the triangle by simple trigonometry.

The transits of Venus over the sun's face are other natural events which seem to give most accurate measurements of the sun's parallax, or apparent difference of position as seen from distant points of the earth's surface. The sun forms a kind of background on which the place of the planet is marked, and serves as a measuring instrument free from all the errors of construction, which affect human instruments. The rotation of the earth, too, by variously affecting the apparent velocity of ingress or egress of Venus, as seen from different places, discloses the amount of the parallax. It has been sufficiently shown that by rightly choosing the moments of observation, the planetary bodies may often be made to reveal their relative distance, to measure their own position, to record their own movements with a high degree of accuracy. With the improvement of astronomical instruments, such conjunctions become less necessary to the progress of the science, but it will always remain advantageous to choose those moments for observation when instrumental errors enter with the least effect.

In other sciences, exact quantitative laws can occasionally be obtained without instrumental measurement, as when we learn the exactly equal velocity of sounds of different pitch, by observing that a peal of bells or a musical performance is heard harmoniously at any distance to which the sound penetrates; this could not be the case, as Newton remarked, if one sound overtook the other. One of the most important principles of the atomic theory, was proved by implication, before the use of the balance was introduced into chemistry. Wenzel observed, before 1777, that when two neutral substances decompose each other, the resulting salts are also neutral. In mixing sodium sulphate and barium nitrate, we obtain insoluble barium sulphate and neutral sodium nitrate. This result could not follow unless the nitric acid, requisite to saturate one atom of sodium, were exactly equal to that required by one atom of barium, so that an exchange could take place without leaving either acid or base in excess [d].

A very important principle of mechanics may also be established by a simple acoustical observation. When a rod or tongue of metal fixed at one end is set in vibration, the pitch of the sound may be observed to be exactly the same, whether the vibrations be small or great; hence the oscillations are isochronous, or equally rapid, independently of their magnitude. On the ground of theory, it can be shown that such a result only happens when the flexure is proportional to the deflecting force. Thus the simple observation that the pitch of the sound of a harmonium, for instance, does not change with its loudness, establishes an exact law of nature[e].

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[d] Daubeny, 'Atomic Theory,' p. 30.

A closely similar instance is found in the proof that the intensity of light or heat rays varies inversely as the square of the distance increases. For the apparent magnitude certainly varies according to this law; hence, if the intensity of light varied according to any other law, the brightness of an object would be different at different distances, which is not observed to be the case. Melloni applied the same kind of reasoning, in a somewhat different form, to the radiation of heat-rays[f].

Modes of Indirect Measurement.

Some of the most conspicuously beautiful experiments in the whole range of science, have been devised for the purpose of indirectly measuring quantities, which in their extreme greatness or smallness surpass the powers of sense. All that we need to do, is to discover some other conveniently measurable phenomenon, which is related in a known ratio or according to a known law, however complicated, with that to be measured. Having once obtained experimental data, there is no further difficulty beyond that of arithmetic or algebraic calculation.

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[e] Jamin, 'Cours de Physique,' vol. i. p. 152.

[f] Balfour Stewart's, 'Elementary Treatise on Heat,' 1st edit. pp. 164, 165.

Gold is reduced by the gold-beater to leaves so thin, that the most powerful microscope would not detect any measurable thickness. If we laid several hundred leaves upon each other to multiply the thickness, we should still have no more than $\frac{1}{100}$th of an inch at the most to measure, and the errors arising in the superposition and measurement would be considerable. But we can readily obtain an exact result through the connected amount of weight. Faraday weighed 2000 leaves of gold, each $3\frac{3}{8}$ inch square, and found them equal to 384 grains. From the known specific gravity of gold, it was easy to calculate that the average thickness of the leaves was $\frac{1}{282.000}$ of an inch[g].

We must ascribe to Newton the honour of leading the way in methods of minute measurement. He did not call waves of light by their right name, and did not understand their nature; yet he measured their length, though it did not exceed the 2,000,000th part of a metre or the one fifty thousandth part of an inch. He pressed together two lenses of very large but known radii. It was not difficult to calculate the interval between the lenses at any point, by measuring the distance from the central point of contact. Now, with homogeneous rays the successive rings of light and darkness mark the points at which the interval between the lenses is equal to one half, or any multiple of half a vibration of the light, so that the length of the vibration became known. In a similar manner many phenomena of interference of rays of light admit of the measurement of the wave lengths. The fringes of interference arise from rays of light which cross each other at a small angle, and an excessively minute difference in the lengths of the waves makes a very perceptible difference in the position of the point at which two rays will interfere and produce darkness.

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[g] Faraday, 'Chemical Researches,' p. 393.

M. Fizeau has recently employed Newton's rings in an inverse manner, to measure small amounts of motion. By merely counting the number of rings of sodium monochromatic light passing a certain point where two glass plates are in close proximity, he is able to ascertain with the greatest accuracy and ease the change of distance between these glasses, produced, for instance, by the expansion of a metallic bar, connected with one of the glass plates[h].

Nothing excites more admiration than the mode in which scientific observers can occasionally measure quantities, which seem beyond the bounds of human observation. We know the *average* depth of the Pacific Ocean to be 14,190 feet, not by actual sounding, which would be impracticable in sufficient detail, but by noticing the rate of transmission of earthquake waves from the South American to the opposite coasts, the rate of movement being connected by theory with the depth of the water[i]. In the same way the average depth of the Atlantic Ocean is inferred to be no less than 22,157 feet, from the velocity of the ordinary tidal waves. A tidal wave again gives beautiful evidence of an effect of the law of gravity, which we could never in any other way detect. Newton estimated that the moon's force in moving the ocean is only $\frac{1}{2,871,400}$ part of the whole force of gravity, which even the pendulum, used with the utmost skill, would fail to render apparent. Yet the immense extent of the ocean allows the accumulation of the effect into a very palpable amount; and from the comparative heights of the lunar and solar tides, Newton roughly estimated the comparative forces of the moon's and sun's gravity at the earth[k].

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[h] 'Proceedings of the Royal Society,' 30th November, 1866.

[i] Herschel, 'Physical Geography,' § 40.

A few years ago it might have seemed impossible that we should ever measure the velocity with which a star approaches or recedes from the earth, since the apparent position of the star is thereby unaltered. But the spectroscope now enables us to detect and even measure such motion with considerable accuracy, by the alteration which it causes in the apparent rapidity of vibration, and consequently in the refrangibility of rays of light of definite colour. And while our estimates of the lateral movements of stars depend upon our very uncertain knowledge of their distance, the spectroscope gives the motion in another direction in absolute quantity, irrespective of all other quantities known or unknown, excepting the motion of the earth itself[l].

The rapidity of vibration for each musical tone, having been accurately determined by comparison with the Syren (p. 12), we can use sounds as indirect indications of rapid vibrations. It is now known that the contraction of a muscle arises from the periodical contractions of each separate fibre, and from a faint sound or susurrus which accompanies the action of a muscle, it is inferred that each contraction lasts for about $\frac{1}{300}$ of a second. Minute quantities of radiant heat are now always measured indirectly by the electricity which they produce when falling upon a thermopile. The extreme delicacy of the method seems to be due to the power of multiplication at several points in the apparatus. The number of elements or junctions of different metals in the thermopile can be increased so that the tension of the electric current derived from the same intensity of radiation is multiplied; the effect of the current upon the magnetic needle can be multiplied within certain bounds, by passing the current many times round it in a coil; the excursions of the needle can be increased by rendering it astatic and increasing the delicacy of its suspension; lastly, the angular divergence can be observed, with any required accuracy, by the use of an attached mirror and distant scale viewed through a telescope (p. 234). Such is the delicacy of this method of measuring heat, that Dr. Joule succeeded in making a thermopile which would indicate a difference of $\frac{1}{8800}$ part of a degree centigrade[m].

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[k] 'Principia,' bk. iii. Prop. 37, 'Corollaries,' 2 and 3. Motte's translation, vol. ii. p. 310.

[l] Roscoe's, 'Spectrum, Analysis,' 1st ed. p. 296.

A striking case of indirect measurement is furnished by the revolving mirror of Wheatstone and Foucault, whereby a minute interval of time is estimated in the form of an angular deviation. Wheatstone viewed an electric spark in a mirror rotating so rapidly, that if the duration of the spark had been more than $\frac{1}{72,000}$ of a second, the point of light would have appeared elongated to an angular extent of one-half degree. In the spark, as drawn directly from a Leyden jar, no elongation was apparent, so that the duration of the spark was immeasurably small; but when the discharge took place through a bad conductor, the elongation of the spark denoted a sensible duration[n]. In the hands of Foucault the rotating mirror gave a measure of the time occupied by light in passing through a few metres of space.

Comparative Use of Measuring Instruments.

In almost every case a measuring instrument serves, and should serve only as a means of comparison between two or more magnitudes. As a general rule, we should not even attempt to make the divisions of the measuring scale exact multiples or submultiples of the unit, but, regarding them as arbitrary marks, should determine their values by comparison with the standard itself. Thus the perpendicular wires in the field of a transit telescope, are fixed at nearly equal but arbitrary distances, and those distances are afterwards determined, as first suggested by Malvasia, by watching the passage of star after star across them, and noting the intervals of time by the clock. Owing to the perfectly regular motion of the earth, these time intervals give an exact determination of the angular intervals. In the same way, the angular value of each turn of the screw micrometer attached to a telescope, can be easily and accurately ascertained.

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[m] 'Philosophical Transactions' (1859), vol. cxlix. p. 94.

[n] Watts' 'Dictionary of Chemistry,' vol. ii. p. 393.

When a thermopile is used to observe radiant heat, it would be almost impossible to calculate on *à priori* grounds what is the value of each division of the galvanometer circle, and still more difficult to construct a galvanometer, so that each division should have a given value. But this is quite unnecessary, because by placing the thermopile before a body of known dimensions, at a known distance, with a known temperature, and radiating power, we measure a known amount of radiant heat, and inversely measure the value of the indications of the thermopile. In a similar way Mr. Joule ascertained the actual temperature produced by the compression of bars of metal. For having inserted a simple thermopile composed of a single junction of copper and iron wire, and noted the deflections of the galvanometer, he had only to dip the bars into water of different temperatures, until he produced a like deflection, in order to ascertain the temperature developed by pressure[o].

[o] 'Philosophical Transactions' (1859), vol. cxlix. p. 119, &c.

In many instances we are indeed obliged to accept a very carefully constructed instrument as a standard, as in the case of a standard barometer. But it is then best to treat all inferior instruments comparatively only, and determine the values of their scales by comparison with the assumed standard.

Systematic Performance of Measurements.

When a large number of accurate measurements have to be effected, it is usually desirable to make a certain number of determinations with scrupulous care, and afterwards use them as points of reference for the remaining determinations. In the trigonometrical survey of a country, the principal triangulation fixes the relative positions and distances of a few points with rigid accuracy. A minor triangulation refers every prominent hill or village to one of the principal points, and then the details are filled in by reference to the secondary points. The survey of the heavens is effected in a like manner. The ancient astronomers compared the right ascensions of a few principal stars with the moon, and thus ascertained their positions with regard to the sun; the minor stars were afterwards referred to the principal stars. Tycho followed the same method, except that he used the more slowly moving planet Venus instead of the moon. Flamsteed was in the habit of using about seven stars, favourably situated at points all round the heavens. The distances of the other stars from these standard points, were determined in his early observations by the use of the quadrant[p]. Even since the introduction of the transit telescope and mural circle, tables of standard stars are formed at Greenwich, the positions being determined with every possible accuracy, so that they can be employed for purposes of reference by all astronomers.

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[p] Baily's 'Account of Flamsteed.' pp. 378–380.

In ascertaining the specific gravities of substances, all gases are referred to atmospheric air at a given temperature and pressure; all liquids and solids are referred to water. We require to compare the densities of water and air with great care, and the comparative densities of any two substances whatever can then be with ease ascertained.

In comparing a very great with a very small magnitude, it is usually desirable to break up the process into several steps, using intermediate terms of comparison. We should never think of measuring the distance from London to Edinburgh by laying down measuring rods throughout the whole distance. A base of several miles in length is selected on level ground, and compared on the one hand with the standard yard, and on the other with the distance of London and Edinburgh, or any other two points, by trigonometrical survey. It would be exceedingly difficult to compare the light of a star with that of the sun, which would be about thirty thousand million times greater; but Sir J. Herschel[q] effected the comparison by using the full moon as an intermediate unit. Wollaston ascertained that the sun gave 801,072 times as much light as the full moon, and Herschel determined that the light of the latter exceeded that of α Centauri 27,408 times, so that we find the ratio between the light of the sun and star to be that of about 22,000,000,000 to 1.

The Pendulum.

By far the most perfect and beautiful of all instruments of measurement is the pendulum. Consisting

[q] Herschel's 'Astronomy,' § 817, 4th. ed. p. 553.

merely of a heavy body suspended freely at an invariable distance from a fixed point, it is the most simple in construction; and yet all the highest problems of physical measurement depend upon its careful use. Its excessive value arises from two circumstances, which render it at once most accurate and indispensable.

(1) The method of repetition is eminently applicable to it, as already described (p. 339.)

(2) Unlike any other instrument, it connects together three different variable quantities, those of space, time, and force.

In most works on natural philosophy it is shown, that when the oscillations of the pendulum are infinitely small, the square of the time occupied by an oscillation is directly proportional to the length of the pendulum, and indirectly proportional to the force affecting it, of whatever kind. The whole theory of the pendulum is contained in the formula, first given by Huyghens in his Horologium Oscillatorium,

$$\text{time of oscillation} = 3{\cdot}14159\,.\;.\;\times \sqrt{\frac{\text{length of pendulum}}{\text{force.}}}$$

The quantity 3·14159 is the constant ratio of the circumference and radius of a circle, and is of course known with accuracy. Hence, any two of the three quantities concerned being given, the third may be found; or any two being maintained invariable, the third will be invariable. Thus a pendulum of invariable length suspended at the same place, where the force of gravity may be considered uniform, furnishes a theoretically perfect measure of time. The same invariable pendulum being made to vibrate at different points of the earth's surface, and the time of vibration being astronomically determined, the force of gravity becomes accurately known. Finally, with a known force of gravity, and time of vibration ascertained by reference to the stars, the length is determinate.

In the first use all astronomical observations depend upon it. In the second employment it has been almost equally indispensable. The primary principle that gravity is equal in all matter was proved by Newton's and Gauss' pendulum experiments. The torsion pendulum of Michell, Cavendish, and Baily, depending upon exactly the same principles as the ordinary pendulum, gave the density of the earth, one of the foremost natural constants. Kater and Sabine, by pendulum observations in different parts of the earth, ascertained the variation of gravity, whence comes a determination of the earth's ellipticity. The laws of electric and magnetic attraction have also been determined by the method of vibrations, which is in constant use in the measurement of the horizontal force of terrestrial magnetism.

We must not confuse with the ordinary use of the pendulum its application by Newton, to show the absence of internal friction against space[r], or to ascertain the laws of motion and elasticity[s]. In such cases the extent of vibration is the quantity measured, and the principles of the instrument are different.

Attainable Accuracy of Measurement.

It is a matter of some interest to compare the degrees of accuracy, which can be attained in the measurement of different kinds of magnitude. Few measurements of any kind are exact to more than six significant figures[t], but it is seldom that such a point of accuracy can be hoped for. Time is the magnitude which seems to be capable of the most exact discrimination, owing to the properties of the pendulum, and the principle of repetition already described (pp. 339, 353). As regards short intervals of time, it has already been stated that Sir George Airy was able to estimate a difference of 2¼ seconds per day, between two pendulums with an uncertainty of less than ·01 of a second, or one part in 8,640,000, an exactness, as he truly remarks, 'almost beyond conception[u]'. The ratio between the mean solar and the sidereal day, too, is known to about one part in one hundred millions, or to the eighth place of decimals (p. 337).

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[r] 'Principia,' bk. ii. Sect. 6. Prop. 31. Motte's Translation, vol. ii. p. 107.

[s] Ibid. bk. i. Law iii. Corollary 6. Motte's Translation, vol. i. p. 33.

[t] Thomson and Tait's 'Natural Philosophy,' vol. i. p. 333.

Determinations of weight seem to come next in exactness, owing to the fact that repetition without error is applicable to them (p. 340). An ordinary good balance should show about one part in 500,000 of the load[x]. The finest balance employed by M. Stas, turned with $\frac{1}{33}$ of a milligramne, when loaded with 25 grammes in each pan, that is, with one part in 825,000 of the load[y]. But balances have certainly been constructed to show one part in a million[z], and Ramsden is commonly said to have constructed a balance for the Royal Society, to indicate one part in seven millions, though this is hardly credible. Professor Clerk Maxwell takes it for granted that one part in five millions can be detected, but we ought to discriminate between what a balance can do when first constructed, and when in continuous use.

Determinations of lengths, unless performed with extraordinary care, are open to much error in the junction of the measuring bars. Even in measuring the base line of a trigonometrical survey, the accuracy generally attained is only that of about one part in 60,000, or an inch in the mile[a]; but it is said that in four measurements of a base line carried out very recently at Cape Comorin, the greatest error was 0·077 inch in 1·68 mile, or one part in 1,382,400, an almost incredible degree of accuracy[b]. Sir J. Whitworth has shown that touch is even a more delicate mode of measuring lengths than sight, and by means of a splendidly executed screw, and a small cube of iron placed between two flat-ended iron bars, so as to be suspended when touching them, he can detect a change of dimension in a bar, amounting to no more than one-millionth of an inch[c].

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[u] 'Philosophical Transactions,' (1856), vol. cxlvi pp. 330–1.

[x] Thomson and Tait, 'Natural Philosophy,' vol. i. p. 333.

[y] 'First Annual Report of the Mint,' p. 106.

[z] Jevons, in Watts' 'Dictionary of Chemistry,' vol. i. p. 483.

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[a] Thomson and Tait, 'Natural Philosophy,' vol. i. p. 333.

[b] 'Athenæum,' February 28, 1870, p. 295.

[c] British Association, Glasgow, 1856. 'Address of the President of the Mechanical Section.'

CHAPTER XIV.

UNITS AND STANDARDS OF MEASUREMENT.

INSTRUMENTS of measurement are, as we have seen, only means of comparison between one magnitude and another, and as a general rule we must assume some one arbitrary magnitude, in terms of which all results of measurement are to be expressed. Mere ratios between any series of objects will never tell us their absolute magnitudes; we must have at least one ratio for each, and we must have one absolute quantity. The number of ratios n are expressible in n equations, which will contain at least $n + 1$ quantities, so that if we employ them to make known n magnitudes, we must have one magnitude known. Hence, whether we are measuring time, space, density, weight, mass, energy, or any other physical quantity, we must refer to some concrete standard, some actual object, which if once lost and irrecoverable, all our measures lose their absolute meaning. This concrete standard is in all, except two, cases absolutely arbitrary in point of theory, and its selection a question of practical convenience.

Of the two cases in which a natural standard unit is ready made for us, one case is that of number itself. Abstract number needs no special unit; for any object by existing or being thought of as separate from other objects (p. 176), furnishes us with a unit, and is the only standard required.

Angular magnitude is the second case in which we have a natural and almost necessary unit of reference, namely, the whole revolution or *perigon*, as it has been called by Mr. Sandeman[a].

It is a necessary result of the uniform properties of space, that all complete revolutions are equal to each other, so that we need not select any one, and can always refer anew to space itself. Whether we take the whole perigon, its half, or its quarter, is really immaterial; Euclid took the right angle, because the Greek geometers had never generalized their notions of angular magnitude sufficiently to conceive clearly angles of all magnitude, or of unlimited *quantity of revolution*. But Euclid defines a right angle as half that made by a line with its own continuation, not called by him an angle, and which is of course equal to half a revolution. In mathematical analysis, again, a different fraction of the perigon is taken, namely, such a fraction that the arc or portion of the circumference included within it is equal to the radius of the circle. This angle, called by De Morgan the *arcual unit*, is equal to about 57°, 17′, 44″·8, or decimally 57°·295779513 . . . . . , and is such that the half revolution contains 3·14159265 . . . . such units[b]. Though this standard angle is naturally employed in mathematical analysis, and any other unit would introduce needless complexity, we must not look upon it as a distinct unit, since its amount is connected with that of the half perigon, by a natural constant 3·14159 . . . . . usually signified by the letter π.

When we pass to other species of quantity, the choice of unit is found to be entirely arbitrary. There is absolutely no mode of defining a length, but by selecting some physical object exhibiting that length between certain obvious points—as, for instance, the extremities of a bar, or marks made upon its surface.

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[a] 'Pelicotetics, or the Science of Quantity; an Elementary Treatise on Algebra, and its groundwork Arithmetic.' By Archibald Sandeman, M.A. Cambridge, (Deighton, Bell, and Co.) 1868, p. 304.

[b] De Morgan's 'Trigonometry and Double Algebra,' p. 5.

Standard Unit of Time.

Time is the great independent variable of all change, that which itself flows on uninterruptedly, and brings the variety which we call life and motion. When we reflect upon its intimate nature, Time, like every other element of existence, proves to be an inscrutable mystery. We can only say with St. Augustin, to one who asks us what is time, 'I know when you do not ask me.' The mind of man will ask what can never be answered, but one result of a true and rigorous logical philosophy must be to convince us, that scientific explanation can only take place between phenomena which have something in common, and that when we get down to primary notions, like those of time and space, the mind must meet a point of mystery beyond which it cannot penetrate. A definition of time must not be looked for; if we say with Hobbes[c], that it is 'the phantasm of before and after in motion,' or with Aristotle that it is 'the number of motion according to former and latter;' we obviously gain nothing, because the notion of time is involved in the expressions *before and after, former and latter.* Time is undoubtedly one of those primary notions which can only be defined physically, or by observation of phenomena which proceed in time.

If we have not advanced a step beyond Augustin's acute reflections on this subject[d], it is curious to observe the wonderful advances which have been made in the practical measurement of its efflux. The rude sun-dial or the rising of a conspicuous star, gave points of reference, while the flow of water from the clepsydra, the burning of a candle, or, in the monastic ages, even the continuous equable chanting of psalms, gave the means of roughly subdividing periods, and marking the hours of the day and night[e]. The sun and stars still furnish the standard of time, but means of accurate subdivision have become requisite, and this has been furnished by the pendulum and the chronoscope. By the pendulum we can accurately divide the day into seconds of time. By the chronograph we can subdivide the second into a hundred, a thousand, or even a million parts. Wheatstone measured the duration of an electric spark, and found it to be no more than $\frac{1}{115,200}$ part of a second, while more recently Captain Noble has been able to appreciate intervals of time, not exceeding the millionth part of a second.

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[c] 'English Works of Thos. Hobbes,' Edit. by Molesworth, vol. i. p. 95.

[d] 'Confessions,' bk. xi. chapters 20–28.

When we come to inquire precisely what phenomenon it is that we thus so minutely measure, we meet insurmountable difficulties. Newton distinguished time according as it was *absolute* or *apparent* time, in the following words :—

'Absolute, true, and mathematical time of itself and from its own nature, flows equably without regard to anything external, and by another name is called *duration;* relative, apparent and common time, is some sensible and external measure of duration by the means of motion[f]'. Though we are perhaps obliged to assume the existence of a uniformly increasing quantity which we call time, yet we cannot feel or know abstract and absolute time. Duration must be made manifest to us by the recurrence of some phenomenon. The succession and change of our own thoughts is no doubt the first and simplest measure of time, but a very rude one, because in some persons and circumstances the thoughts evidently flow with much greater rapidity than in other persons and circumstances. In the absence of all other phenomena, the interval between one thought and another, would necessarily become the unit of time. The earth, as I have already said, is the real clock of the astronomer, and is practically assumed as invariable in its movements. But on what ground is it so assumed? According to the first law of motion, every body perseveres in its state of rest or of uniform motion in a right line, unless it is compelled to change that state by forces impressed thereon. Rotatory motion is subject to a like condition, namely, that it perseveres uniformly unless disturbed by extrinsic forces. Now uniform motion means motion through equal spaces in equal times, so that if we have a body entirely free from all resistance or perturbation, and can measure equal spaces of its path, we have a perfect measure of time. But let it be remembered at the same time, that this law has never been absolutely proved by experience; for we cannot point to any body, and say that it is wholly unresisted or undisturbed; and even if we had such a body, we should need some entirely independent standard of time to ascertain whether its motion was really uniform. As it is in moving bodies that we find the best standard of time, we cannot theoretically speaking use them to prove the uniformity of their own movements, which would amount to a *petitio principii.* Our experience amounts to this, that when we examine and compare the movements of bodies which seem to us nearly free from disturbance, we find them give nearly harmonious measures of time. If any one body which seems to us to move uniformly is not doing so, but is subject to fits and starts unknown to us, because we have no absolute standard of time, then all other bodies must be subject to exactly the same arbitrary fits and starts, otherwise there would be a discrepancy between them disclosing the irregularities. Just as in comparing together a number of chronometers, we should soon detect bad ones by their irregular going, as measured by the others, so in nature we detect disturbed movement by its discrepancy from that of other bodies, which we believe to be undisturbed, and which agree very nearly among themselves. But inasmuch as the measure of motion involves time, and the measure of time involves motion, there must be ultimately an assumption. We may define equal times, as times during which a moving body under the influence of no force describes equal spaces[g], but all we can say in its support is, that it leads us into no known difficulties, and that to the best of our experience, one freely moving body gives exactly the same results as any other.

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[e] Sir G. C. Lewis gives many curious particulars concerning the measurement of time, 'Astronomy of the Ancients,' pp. 241, &c.

[f] 'Principia,' bk. i. 'Scholium to Definitions.' Translated by Motte, vol. i. p. ix. See also, p. 11.

When we inquire where the freely moving body is, no satisfactory answer can be given. Practically the rotating globe is sufficiently accurate, and Thomson and Tait say: 'Equal times are times during which the earth turns through equal angles[h]'. No long time has passed since astronomers thought it impossible to detect any inequality in its movement. Poisson was supposed to have proved that a change in the length of the sidereal day, amounting to one ten-millionth part in 2500 years, was incompatible with an ancient eclipse recorded by the Chaldæans, and similar calculations were made by Laplace. But it is now known that these calculations were somewhat in error, and that the dissipation of energy arising out of the friction of tidal waves, and the radiation of the heat into space, has slightly decreased the rapidity of the earth's rotatory motion. The sidereal day is now longer by one part in 2,700,000, than it was in 720 B.C. Even before this discovery, it was certain that the invariable rotation depended upon the perfect maintenance of the earth's internal heat, which is requisite in order that the earth's dimensions shall be unaltered. Now the earth being far superior in temperature to empty space, must cool more or less rapidly, so that it cannot furnish an absolute measure of time. Similar objections could be raised to all other rotating bodies within our cognizance.

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[g] Rankine, 'Philosophical Magazine,' Feb. 1867, vol. xxxiii. p. 91.

[h] 'Treatise on Natural Philosophy,' vol. i. p. 179.

The moon's motion round the earth, and the earth's motion round the sun, form the next best measure of time. They are subject, indeed, to all kinds of disturbance from other planets, but it is believed that these must in the course of time run through their rhythmical courses, and leave the mean distances unaffected, and consequently, by the third Law of Kepler, the periodic times unchanged. But there is more reason than not to believe that the earth encounters a certain slight resistance in passing through space, like that which is so apparent in Encke's comet. There may also be a certain dissipation of energy in the electrical relations of the earth to the sun, possibly identical with that which is manifested in the retardation of comets[i]. It is probably an untrue assumption then, that the earth's orbit remains quite invariable, and if so our last hope of getting a really uniform measure of time disappears, and we are reduced to accepting such as are sufficient for all practical purposes.

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[i] 'Proceedings of the Manchester Philosophical Society,' 28th Nov. 1871, vol. xi. p. 33.

It is just possible that in the course of time, some other body may be found to furnish a better standard of time than the earth in its annual motion. The greatly superior mass of Jupiter and its satellites, and their greater distance from the sun, may render the electrical dissipation of energy less considerable even than in the case of the earth. But the choice of the best measure will always be an open one, and whatever moving body we assume, may ultimately be shown to be subject to disturbing forces.

The pendulum, although so admirable an instrument for subdivision of time, entirely fails as a standard; for though the same pendulum affected by the same force of gravity would perform equal vibrations in equal times, yet the slightest change in the form or weight of the pendulum, the slightest corrosion of any part, or the most minute displacement of the point of suspension, would falsify the results, and there enter many other difficult questions of temperature, resistance, length of vibration, &c.

Thomson and Tait are of opinion[k] that the ultimate standard of chronometry must be founded on the physical properties of some body of more constant character than the earth; for instance, a carefully arranged metallic spring, hermetically sealed in an exhausted glass vessel. Although their suggestion is no doubt theoretically correct, it is hard to see how we can be sure that the dimensions and elasticity of a piece of wrought metal will remain perfectly unchanged for the few millions of years contemplated by them. A nearly perfect gas, like hydrogen, is perhaps the only kind of substance in the unchanged elasticity of which we could have confidence. Moreover, it is difficult to perceive how the undulations of such a spring could be observed with the requisite accuracy. We thus appear to be devoid of any hope of establishing a sure standard of the efflux of time.

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[k] 'The Elements of Natural Philosophy,' part i. p. 119.

The Unit of Space and the Bar Standard.

Next in importance after the measurement of time is that of space. Time comes first in theory, because phenomena, our internal thoughts for instance, may change in time without regard to space magnitude. As to the phenomena of outward nature, they tend more and more to resolve themselves into the motion of molecules, and motion cannot be conceived or measured without reference both to time and space.

Turning now to space measurements, we find it almost equally difficult to fix and define once and for ever, a unit magnitude. There are three different modes in which it has been proposed to attempt the perpetuation of a standard length.

(1) By constructing an actual specimen of the standard yard or metre, in the form of a bar.

(2) By assuming the globe itself to be the ultimate standard of magnitude, the practical unit being a submultiple of some dimension of the globe.

(3) By adopting the length of a simple pendulum, beating seconds as a standard of reference.

At first sight it might seem that there was no great difficulty in this matter, and that any one of these methods might serve well enough; but the more minutely we inquire into the details, the more hopeless appears to be the attempt to establish an invariable standard. We must in the first place point out a principle not of an obvious character, namely, that *the standard length must be defined by one single object*[1]. To make two bars of exactly the same length, or even two bars bearing a perfectly defined ratio to each other, is beyond the power of human art. If two copies of the standard metre be made and declared equally correct, future investigators will certainly discover some discrepancy between them, proving of course that they cannot both be the standard, and giving cause for dispute as to what magnitude should then be taken as correct.

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[1] See Harris' 'Essay upon Money and Coins,' part ii. [1758] p. 127.

If one invariable bar could be constructed and maintained as the absolute standard, no such inconvenience could arise. Each successive generation as it acquired higher powers of measurement, would detect errors in the copies of the standard, but the standard itself would be unimpeached, and would, as it were, become by degrees more and more accurately known. Unfortunately to construct and preserve a metre or yard is also a task which is either impossible, or what comes nearly to the same thing, cannot be shown to be possible. Passing over the practical difficulty of defining the ends of the standard length with complete accuracy, whether by dots or lines on the surface, or by the terminal points of the bar, we have no means of proving that substances remain of invariable dimensions. Just as we cannot tell whether the rotation of the earth is uniform, except by comparing it with other moving bodies, believed to be more uniform in motion, so we cannot detect the change of length in a bar, except by comparing it with some other bar supposed to be invariable. But how are we to know which is the invariable bar? It is certain that many rigid and apparently invariable substances do change in dimensions. The bulb of a thermometer certainly contracts by age, besides undergoing rapid changes of dimensions when warmed or cooled through 100° Cent.[m] Can we be sure that even the most solid metallic bars do not slightly contract by age, or undergo variations in their structure by change of temperature. M. Fizeau was induced to try whether a quartz crystal, subjected to several hundred alternations of temperature, would be modified in its physical properties, and he was unable to detect any change in the coefficients of expansion[n]. It does not follow however, that, because no apparent change was discovered in a quartz crystal, newly-constructed bars of metal would undergo no change.

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[m] Watts' 'Dictionary of Chemistry,' vol. v. pp. 766, 767. Dr. Joule has recently confirmed the statements concerning the contraction of a thermometer-bulb.

The only principle, as it seems to me, upon which the perpetuation of a standard of length can be ultimately rested, is that, if a variation of length occurs, it will in all probability be of different amount in different substances. If then a great number of standard metres were constructed of all kinds of different metals, alloys; hard rocks, such as granite, serpentine, slate, quartz, limestone; artificial substances, such as porcelain, glass, &c., &c., careful comparison would show from time to time the comparative variations of length of these different substances. The most variable substances would be the most divergent, and the true standard would be furnished by the mean length of those which agreed most closely with each other, just as uniform motion is that of those bodies which agree most closely in indicating the efflux of time.

The Terrestrial Standard.

The second method assumes that the globe itself is a body of invariable dimensions. The founders of the metrical system selected the ten-millionth part of the distance from the equator to the pole as the definition of the metre, and the late Sir John Herschel proposed[o] that the English inch, which is now almost exactly the 500,500,000th part of the polar axis of the earth, should be made exactly equal to the 500,000,000th part, and be adopted as our standard. The first imperfection in such a method is that the earth is certainly not invariable in size; for we know that it is superior in temperature to surrounding space, and must be slowly cooling and contracting. There is much reason to believe that all earthquakes, volcanoes, mountain elevations, and changes of sea level, are evidences of this contraction as asserted by Mr. Mallet[p]. But such is the vast bulk of the earth and the duration of its past existence, that this contraction is perhaps less rapid in proportion than that of any bar or other material standard which we can construct.

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[n] 'Philosophical Magazine,' (1868), 4th Series, vol. xxxvi. p. 32.

[o] 'Familiar Lectures on Scientific Subjects,' (1866) p. 191.

The second and chief difficulty of this method arises from the vast size of the earth, which prevents us from making any comparison with the ultimate standard, except by a trigonometrical survey of a most elaborate and costly kind. The French physicists, who first proposed the method, attempted to obviate this inconvenience by carrying out the survey once for all, and then constructing a standard metre, which should be exactly the one ten millionth part of the distance from the pole to the equator. But since all measuring operations are merely approximate, as so often stated in previous pages, it was impossible that this operation could be perfectly achieved. Accordingly it was shown by Colonel Puissant in 1838, that the supposed French metre was erroneous to the considerable extent of one part in 5527, the quadrant of the earth's circumference measuring 10,001,789 instead of 10,000,000 of such metres. It then became necessary either to alter the length of the assumed metre, or otherwise to abandon its supposed relation to the earth's dimensions.

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[p] 'Proceedings of the Royal Society,' 20th June, 1872, vol. xx. p. 438.

The French Government and the present International Metrical Commission have for obvious reasons decided in favour of the latter course, and have thus reverted to the first method of defining the metre by a given bar. As from time to time the ratio between this assumed standard metre and the dimensions of the earth becomes more and more accurately known, we have the better means of restoring that metre by actual reference to the globe if required. But until lost, destroyed, or for some clear reason discredited, the bar metre and not the globe is the standard. Any of the more accurate measurements of the English trigonometrical survey might in like manner be employed to restore our standard yard, in terms of which the results are recorded[q].

The Pendulum Standard.

The third method of defining a standard length, by reference to the seconds' pendulum, was first proposed by Huyghens, and was at one time adopted by the English Government. From the principle of the pendulum (p. 353) it clearly appears that if the time of oscillation and the force actuating the pendulum be the same, the length must be the same. We do not get rid of theoretical difficulties, for we must practically assume the attraction of gravity at some point of the earth's surface, say London, to be unchanged from time to time, and the sidereal day to be invariable, neither assumption being absolutely correct so far as we can judge. The pendulum, in short, is only an indirect means of making one physical quantity of space depend upon two other physical quantities of time and force.

The practical difficulties are, however, of a far more serious character than the theoretical ones. The length of a pendulum is not the ordinary length of the instrument, which might be greatly varied, without affecting the duration of a vibration, but the distance from the centre of suspension to the centre of oscillation. There is no direct means of determining this centre, which depends upon the average momentum of all the particles of the pendulum as regards the centre of suspension. Huyghens discovered that the centres of suspension and oscillation are interchangeable, and Captain Kater pointed out that if a pendulum vibrates with exactly the same rapidity when suspended from two different points, the distance between these points is the true length of the equivalent simple pendulum[r]. But the practical difficulties in employing Kater's reversible pendulum are considerable, and questions regarding the disturbance of the air, the force of gravity or even the interference of electrical attractions have to be entertained. It has been shown that all the experiments made under the authority of government for establishing the ratio between the standard yard and the seconds' pendulum, were vitiated by an error in the corrections for the resisting, adherent or buoyant power of the air in which the pendulum swung. Even if such corrections were rendered unnecessary by operating in a vacuum, other difficult questions remain[s]. Gauss' mode of comparing the vibrations of a wire pendulum when suspended at two different lengths is open to equal or greater practical difficulties. Thus it is found that the pendulum standard cannot compete in accuracy and certainty with the simple bar standard, and the method would only be useful as an accessory mode of restoring the bar standards if at any time again destroyed.

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[q] Thomson and Tait's 'Elements of Natural Philosophy,' Part 1. p. 119.

[r] Kater's 'Treatise on Mechanics,' Cabinet Cyclopædia, p. 154.

[s] Grant's 'History of Physical Astronomy,' p. 156.

Unit of Density.

Before we can measure and define the phenomena of nature, we require a third independent unit, which shall enable us to define the quantity of matter which occupies any given space. All the motions and changes of nature, as we shall see, are probably so many manifestations of energy; but energy requires some substratum or material machinery of molecules, in and by which it may be exerted. Very little observation shows that, as regards force, there may be two modes of variation of matter. The force required to set a body in motion, varies in simple proportion to the bulk or cubic dimensions of the matter, but also according to its quality. Two cubic inches of iron of uniform quality, will require twice as much force to produce a certain velocity in a given time as one cubic inch; but one cubic inch of gold will require more force than one cubic inch of iron. There is then some new measurable quality in matter apart from its bulk, which we may call *density*, and which is, strictly speaking, indicated by its capacity to resist and absorb the action of force. For the unit of density we may assume that of any substance which is uniform in quality, and can readily be referred to from time to time. Pure water at any definite temperature, for instance that of snow melting under an inappreciable pressure, furnishes a natural and invariable standard of density, and by testing equal bulks of various substances compared with a like bulk of ice-cold water, as regards the velocity produced in a unit of time by the same force, we should ascertain the densities of those substances as expressed in that of water.

Practically the force of gravity is used to measure density; for a simple and beautiful experiment with the pendulum, performed by Newton and Gauss, shows that all kinds of matter equally gravitate, that is, the attractive power of a substance is exactly proportional to its density. Two portions of matter then which are in equilibrium in the balance, may be assumed to possess equal inertia, and their densities will therefore be inversely as their cubic dimensions.

Unit of Mass.

Multiplying the number of units of density of a portion of matter, by the number of units of space occupied by it, we arrive at the quantity of matter, or, as it is usually called, the units of *mass*, as indicated by the inertia and gravity it possesses. To proceed in the most simple and logical manner, the unit of mass ought to be that of a cubic unit of matter of the standard density. The founders of the French metrical system took as their unit of mass, the cubic centimetre of water, at the temperature of maximum density (about 4° Centigrade). They called this unit of mass the *gramme*, and constructed standard specimens of the kilogram, which might be readily referred to by all who required to employ accurate weights. Unfortunately, however, the determination of the bulk of a given weight of water at a certain temperature is an operation involving many practical and theoretical difficulties, and it can not be performed in the present day with a greater exactness than that of about one part in 5000, the results of careful observers being sometimes found to differ as much as one part in 1000[t].

Weights, on the other hand, can be compared with each other to at least one part in a million. Hence if different specimens of the kilogram be prepared by direct weighing against water, they will not agree very closely with each other; and, as a matter of fact, the two principal standard kilograms neither agree with each other, nor with their true definition[u]. The so-called Kilogram des Archives weighs 15432·34874 grains according to Prof. W H. Miller, while the kilogram deposited at the Ministry of the Interior in Paris, as the standard for commercial purposes, weighs 15432·344 grains[x].

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[t] Clerk Maxwell's 'Theory of Heat,' p. 79.

Now since a standard weight constructed of platinum, or platinum and iridium, can be preserved in all probability free from any appreciable alteration, and since it can be very accurately compared with other weights, we shall ultimately attain the greatest exactness in our recorded measurements of weight and mass, by assuming some single standard kilogram as a provisional standard, leaving the determination of its actual mass in units of space and density for future investigation. This is what is practically done at the present day, and thus a unit of mass takes the place of the unit of density, both in the French and the present English systems. The English pound is defined by a certain lump of platinum, carefully preserved at Westminster, and is an entirely arbitrary mass, made to agree as nearly as possible with old English pounds. The gallon, the old English unit of cubic measurement, is defined by the condition that it shall contain exactly ten pounds weight of water at 62° Fahr.; and although it is stated that it has the capacity of about 277·274 cubic inches, this ratio between the cubic and linear system of measurement is not legally enacted, but is left open to investigation from time to time. While the French metric system as originally designed was theoretically perfect, it does not seem to differ practically in this point from the English system.

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[u] Thomson and Tait's 'Treatise on Natural Philosophy,' vol. i. p 325.

[x] Ibid.

Subsidiary Units.

Having once established the standard units of time, space, and density or mass, we might employ them for the expression of all quantities of such nature. But it is often found convenient in particular branches of science, to use multiples or submultiples of the original units, for the expression of quantities, in a clear and simple manner. We use the mile rather than the yard when treating of the magnitude of the globe, and the mean distance of the earth and sun is not too large a unit when we have to describe the distances of the stars. On the other hand, when we are occupied with microscopic objects, the inch, the line or the millimetre, become the most convenient terms of expression.

It is allowable for a scientific man to introduce a new unit in any branch of knowledge, provided that it assists precise expression, and is carefully brought into relation with the primary units. Thus Prof. A. W. Williamson has proposed as a convenient unit in chemical science, an absolute volume equal to about 11·2 litres, representing the bulk of one gramme of hydrogen gas at standard temperature and pressure, or the *equivalent* weight of any other gas, such as 16 grammes of oxygen, 14 grammes of nitrogen, &c.; in short, the bulk of that quantity of any one of those gases which weighs as many grammes as there are units in the number expressing its atomic weight[y]. Professor Hofmann has also proposed a new concrete unit for chemists, called a *crith*, to be defined by the weight of one cubic decimetre or litre of hydrogen gas at 0° C. and 0°·76mm., weighing about 0·0896 grammes[z]. Both these units if adopted must be regarded as purely subordinate units, ultimately defined by reference to the primary units, and not involving any new assumption.

[y] 'Chemistry for Students,' by A. W. Williamson. Clarendon Press Series, 2nd ed. Preface p. vi.

[z] 'Introd. to Chemistry,' p. 131.

Derived Units.

The standard units of time, space, and mass having been once fixed, it becomes obvious that many kinds of magnitude are naturally measured by units immediately derived from one or more of the three principal ones. From the standard metre of linear magnitude follows in the most obvious manner the centaire or square metre, the unit of superficial magnitude, and the litre or cube of the tenth part of a metre, the standard of capacity or volume. Velocity of motion, again, is expressed by the ratio of the space passed over, when the motion is uniform, to the time occupied; hence the unit velocity will be that of a body which passes over a unit of space in a unit of time, say one metre per second. Momentum is measured by the mass moving, regard being paid both to the amount of matter and the velocity at which it is moving. Hence the unit of momentum will be that of a unit volume of matter of the unit density moving with the unit velocity, or in the French system, a cubic centimetre of water of the maximum density moving one metre per second.

An accelerating force is measured by the ratio of the momentum generated to the time occupied, the force being supposed to act uniformly. The unit of force will therefore be that which generates a unit of momentum in a unit of time, or which causes, in the French system, one cubic centimetre of water at maximum density to acquire in one second a velocity of one metre per second. The force of gravity is the most familiar kind of force, and as when acting unimpeded upon any substance it produces in a second a velocity of 9·80868 .... metres per second in Paris, it follows that the absolute unit of force is about the tenth part of the force of gravity. If we employ British weights and measures, the absolute unit of force is represented by the gravity of about half an ounce, since the force of gravity of any portion of matter acting upon that matter during one second, produces a final velocity of 32·1889 feet per second or about 32 units of velocity. Although from its perpetual presence and approximate uniformity we find in gravity the most convenient force for reference, and thus habitually employ it to estimate quantities of matter or mass, we must remember that it is only one of many instances of force. Strictly speaking, we should express weight in terms of force, but practically we express all forces in terms of weight.

We still require the unit of energy, a more complex notion. The momentum of a body expresses the quantity of motion which belongs or would belong to the aggregate of the particles, but when we consider how this motion is related to the action of a force producing or removing it, we find that the effect of a force is proportional to the mass multiplied by the square of the velocity and it is most convenient to take half this product as the expression required. But it is shown in books upon Dynamics that it will be exactly the same thing if we define energy by a force acting through a certain space. The natural unit of energy will then be that which overcomes a unit of force acting through a unit of space; when we lift one kilogram through one metre, against gravity, we therefore accomplish 9·80868 . . . . units of work, that is, we turn so many units of potential energy existing in the muscles, into potential energy of gravitation. In lifting one pound through one foot there is in like manner a conversion of 32·1889 units of energy. Accordingly the unit of energy will be that required to lift a kilogram through about one tenth part of a metre against gravity, or, in the English system; to lift one pound through the thirty-second part of a foot.

Every person is at perfect liberty to measure and record quantities in terms of any unit which he likes to adopt. He may use the yard for linear measurement and the litre for cubic measurement, only there will then be a complicated relation between his different results. The system of derived units which we have been briefly considering, is that which gives the most simple and natural relation between quantitative expressions of different kinds, and therefore conduces to ease of comprehension and saving of laborious calculation.

Provisionally Independent Units.

Ultimately, as we can hardly doubt, all phenomena will be recognised as so many manifestations of energy; and, being expressed in terms of the unit of energy, will be referable to the primary units of space, time, and mass. To effect this reduction, however, in any particular case, we must not only be able to compare different quantities of the phenomenon, but to trace the whole series of steps by which it is connected with the primary notions. We can readily observe that the intensity of one source of light is greater than that of another; and, knowing that the intensity of light decreases as the square of the distance, we can easily determine their comparative brilliance. Hence we can express the intensity of light falling upon any surface, if we have a unit in which to make the expression. Light is undoubtedly one form of energy, and the unit ought therefore to be the unit of energy. But at present it is quite impossible to say how much energy there is in any particular amount of light. The question then arises,—Are we to defer the measurement of light until we can fully and accurately assign its relation to other forms of energy? If we answer Yes, it is equivalent to saying that the science of light must stand still perhaps for a generation;

and not only this science but almost every other. The true course evidently is to select, as the provisional unit of light, some light of convenient intensity, which can be reproduced from time to time in exactly the same intensity, and which is defined by physical circumstances. All the phenomena of light may be experimentally investigated relatively to this unit, for instance that obtained after much labour by Bunsen and Roscoe[a]. In after years it will become a matter of inquiry what is the energy exerted in such unit of light; but it may be long before the relation is exactly determined.

A provisionally independent unit, then, means one which is assumed and physically defined in a safe and reproducible manner, in order that particular quantities may be compared *inter se* more accurately than they can yet be referred to the primary units. In reality almost all our measurements are made by such independent units. Even the unit of mass is practically an independent one, as we have seen (p. 373).

Similarly the unit of heat ought to be simply the unit of energy, already described. But a weight can be measured to the one-millionth part, and temperature to less than the thousandth part of a degree Fahrenheit, and to less therefore than the five-hundredth thousandth part of the absolute temperature, whereas the mechanical equivalent of heat is probably not known to the thousandth part. Hence the need of a provisional unit of heat, which is often taken as that requisite to raise a unit weight of water (say one gramme) through one degree Centigrade of temperature, that is from 0° to 1°. This quantity of heat is capable of approximate expression in terms of time, space, and mass; for by the natural constant, determined by Dr. Joule, and called the mechanical equivalent of heat, we know that the assumed unit of heat is equal to the energy of 423·55 gramme-metres, or that energy which will raise the mass of 423·55 grammes through one metre against 9·80868 absolute units of force. Heat may also be expressed in terms of the quantity of ice at 0° Cent., which it is capable of converting into water under an inappreciable pressure.

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[a] 'Philosophical Transactions' (1859), vol. cxlix. p. 884, &c.

The science of electricity has lately become so much a matter of quantity, that it is necessary to have some means of accurate expression. When we know exactly the mechanical equivalent of electricity, we can express quantities of electricity in terms of energy, but in the meantime we need some easy available unit. The British Association accordingly have selected as the unit of electrical force that which can just overcome the resistance offered by a piece of pure silver wire 1 metre in length, and 1 millemetre in diameter. This unit must be regarded as merely a convenient provision for working purposes, to be employed for the easy expression of quantities not yet brought into precise relation with the ultimate standards of time, space, and mass. There may also be other provisionally independent units employed in electrical science, such as the voltametric unit of current strength, namely, that current which by decomposing water produces one cubic centimetre of detonating gas at 0° Cent. and 760 mm. of pressure in one minute. The unit of electrical quantity, again, is that quantity which when concentrated in a point and acting on an equal quantity also concentrated in a point at a unit of distance, exerts a repulsion equal to the unit of force. There must also be a unit of electro-magnetic force. All these electrical units must, however, be definitely related to each other, and to the fundamental units, and it is a matter for continual investigation to determine such relations more and more accurately.

Natural Constants and Numbers.

Having acquired accurate measuring instruments, and decided upon the units in which the results shall be estimated and expressed, there remains the question, What use shall be made of our powers of measurement? Our principal object must be to discover general quantitative laws of nature; but a very large amount of preliminary labour is employed in the accurate determination of the dimensions of existing objects, and the numerical relations between diverse forces and phenomena. Step by step every part of the material universe is surveyed and brought into known relations with other parts. Each manifestation of energy is correlated with each other kind of manifestation. Professor Tyndall has described the care with which such operations are conducted[b].

'Those who are unacquainted with the details of scientific investigation, have no idea of the amount of labour expended on the determination of those numbers on which important calculations or inferences depend. They have no idea of the patience shown by a Berzelius in determining atomic weights; by a Regnault in determining coefficients of expansion; or by a Joule in determining the mechanical equivalent of heat. There is a morality brought to bear upon such matters which, in point of severity, is probably without a parallel in any other domain of intellectual action.'

Every new natural constant which is recorded brings many fresh inferences within our power. For if n be the number of such constants known, then $\frac{1}{2}(n^2-n)$ is the number of ratios which are within our powers of calculation, and this increases with the square of n. We thus gradually piece together a map of nature, in which the lines of inference from one phenomenon to another rapidly grow in complexity, and the powers of scientific prediction are correspondingly augmented.

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[b] Tyndall's 'Sound,' 1st ed. p. 26.

The late Mr. Babbage[c] proposed the formation of a complete collection of all the constant numbers of nature; but such a collection would be almost coextensive with the whole mass of scientific literature. Almost all numbers occurring in works on Chemistry, Mineralogy, Physics, Astronomy, &c. are natural constants, and it would be impracticable to give in any one work more than a selection of the more important numbers.

Our present object will be to classify these constant numbers roughly, according to their comparative generality and importance, under the following heads:—

(1) Mathematical constants.
(2) Physical constants.
(3) Astronomical constants.
(4) Terrestrial numbers.
(5) Organic numbers.
(6) Social numbers.

Mathematical Constants.

At the head of the list of natural constants must come those which express the necessary relations of numbers to each other. The ordinary Multiplication Table is the most familiar and the most important of such series of constants, and is, theoretically speaking, infinite in extent. Next we must place the Arithmetical Triangle, the significance of which has already been pointed out (p. 206.) Tables of logarithms also contain vast series of natural constants, arising out of the relations of pure numbers. At the base of all logarithmic theory is the mysterious natural constant commonly denoted by E, *e*, or ϵ, being equal to the infinite series $1 + \frac{1}{1} + \frac{1}{1.2} + \frac{1}{1.2.3} + \frac{1}{1.2.3.4} + \dots,$

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[c] British Association, Cambridge, 1833. Report, pp. 484–490.

and thus consisting of the sum of the ratios between the numbers of permutations and combinations of 0, 1, 2, 3, 4, &c. things.

Tables of prime numbers and of the factors of composite numbers must not be forgotten.

Another vast and in fact infinite series of numerical constants contains those connected with the measurement of angles, and embodied in trigonometrical tables, whether as natural or logarithmic sines, cosines, and tangents. It should never be forgotten that though these numbers find their chief employment in connexion with trigonometry, or the measurement of the sides of a right-angled triangle, yet the numbers themselves arise out of simple numerical relations bearing no special relation to space.

Foremost among trigonometrical constants is the well known number π, usually employed as expressing the ratio of the circumference and the diameter of a circle; from π follows the value of the arcual or natural unit of angular value as expressed in ordinary degrees (see p. 358).

Among other mathematical constants not uncommonly used may be mentioned tables of factorials (p. 202), tables of Bernouilli's numbers, tables of the error function[d], which latter are indispensable not only in the theory of probability but also in several other branches of science.

It should also be clearly understood that the mathematical constants and tables of reference already in our possession, although very extensive, are only an infinitely small part of what might be formed. With the progress of science the tabulation of new functions will be continually demanded, and it is worthy of consideration whether public money should not be constantly available to reward the enormous labour which must be undertaken in these calculations. Such labours once successfully completed must benefit the whole human race as long as it shall exist. A valuable account of all the chief mathematical tables yet published will be found in De Morgan's article on *Tables*, in the 'English Cyclopædia,' Division of Arts and Sciences, vol. vii. p. 976.

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[d] See J. W. L. Glaisher, 'Philosophical Magazine,' 4th Series, vol. xlii. p. 421.

Physical Constants.

The second class of constants contains those which refer to the actual constitution of matter. For the most part they depend upon the peculiarities of the chemical substance in question, but we may begin with those which are of the most general character. In a first sub-class we may place the velocity of light or heat undulations, the numbers expressing the relation between the lengths of the undulations, and the rapidity of the undulations, these numbers depending only on the properties of the ethereal medium, and being probably the same in all parts of the universe. The theory of heat gives rise to several numbers of the highest importance, especially Joule's mechanical equivalent of heat, the absolute zero of temperature, the mean temperature of empty space, &c.

Taking into account the diverse properties of the elements we must have tables of the atomic weights, the specific heats, the specific gravities, the refractive powers, not only of the elements, but their almost infinitely numerous compounds. The properties of hardness, elasticity, viscosity, expansion by heat, conducting powers for heat and electricity, must also be determined in immense detail. There are, however, certain of these numbers which stand out prominently because they serve as intermediate units or terms of comparison. Such are, for instance, the absolute coefficients of expansion of air, water, and mercury, the temperature of the maximum density of water (39°·101 Fahr. or 4°·0 Cent.), the latent heats of water and steam, the boiling-point of water under standard pressure, the melting and boiling-points of mercury, and so on.

Astronomical Constants.

The third great class consists of numbers possessing far less generality because they refer, not to the universal properties of matter, but to the special forms and distances in which matter has been disposed in the part of the universe open to our examination. We have, first of all, to define the magnitude and form of the earth, its mean density, the constant of aberration of light expressing the relation between the earth's mean velocity in space and the velocity of light. From the earth, as our observatory, we then proceed to lay down the mean distances of the sun, and of the planets from the same centre; all the elements of the planetary orbits, the magnitudes, densities, masses, periods of axial rotation of the several planets are by degrees determined with growing accuracy. The same labours must be gone through for the satellites. Catalogues of comets with the elements of their orbits, as far as ascertainable, must not be omitted.

From the earth's orbit as a new base of observations, we next proceed to survey the heavens and lay down the apparent positions, magnitudes, motions, distances, periods of variation, &c. of the stars. All catalogues of stars from those of Hipparchus and Tycho, are full of numbers expressing rudely the conformation of the visible universe. But there is obviously no limit to the labours of astronomers; not only are millions of distant stars awaiting their first measurements, but those already registered require endless scrutiny as regards their movements in the three dimensions of space, their periods of revolution, their changes of brilliancy and colours. It is obvious that though astronomical numbers are conventionally called *constant*, they are in all cases probably subject to more or less rapid variation.

Terrestrial Numbers.

Our knowledge of the globe we inhabit involves many numerical determinations, which have little or no connexion with astronomical theory. The extreme heights of the principal mountains, the mean elevation of continents, the mean or extreme depths of the oceans, the specific gravities of rocks, the temperature of mines, all the host of numbers expressing the meteorological or magnetic conditions of every part of the surface must fall into this class. Many of such numbers are hardly to be called constant, being subject to periodic or even secular changes, but they are no more variable in fact than many which in astronomical science are set down as constant. In many cases quantities which seem most variable may go through rhythmical changes resulting in a nearly uniform average, and it is only in the long progress of physical investigation that we can hope to discriminate successfully between those elemental numbers which are absolutely fixed and those which vary. In the latter case the law of variation becomes the constant relation which is the object of our search.

Organic Numbers.

All the forms and properties of brute nature having been sufficiently defined by the previous classes of numbers, the organic world, both vegetable and animal, remains outstanding, and offers a higher series of phenomena for our investigation. All exact knowledge relating to the forms and sizes of living things, their numbers, the quantities of various compounds which they consume, contain, or excrete, their muscular or nervous energy, &c. must be placed apart in a class by themselves. All such numbers are doubtless more or less subject to variation, and but in a minor degree capable of exact determination. Man, so far as he is an animal, and as regards his physical form, must also be treated in this class.

Social Numbers.

Little or no allusion has hitherto been made in this work to the fact that man in his economical, sanitary, intellectual, æsthetic, or moral relations may become the subject of exact sciences, the highest and most useful of all sciences. Every one who is in any degree engaged in statistical inquiry or study must so far acknowledge the possibility of natural laws governing such statistical facts. Hence we must certainly allot a distinct place to all numerical information relating to the numbers, ages, physical and sanitary condition, mortality, of all different peoples, in short, to vital statistics. Economic statistics, comprehending the quantities of commodities produced, existing, exchanged, and consumed, constitute another most extensive body of science. In the progress of reason exact investigation may possibly subdue regions of phenomena which at present defy all analysis and scientific treatment. That scientific method can ever exhaust the phenomena of the human mind is on the other hand incredible.

CHAPTER XV.

ANALYSIS OF QUANTITATIVE PHENOMENA.

In the two preceding chapters we have been engaged in considering how a phenomenon may be accurately measured and expressed. So delicate and complex an operation is a measurement which pretends to any considerable degree of exactness, that no small part of the skill and patience of physicists is usually spent upon this operation. Much of this difficulty arises from the fact that it is scarcely ever possible to measure one simple phenomenon at a time. The ultimate object must be to discover the mathematical equation or law connecting a quantitative cause with its quantitative effect; this purpose usually involves, as we shall see, the varying of one condition at a time, the other conditions being maintained constant. The labours of the experimentalist would be comparatively light if he could carry out this rule of varying one circumstance at a time. He would then obtain a series of corresponding values of the variable quantities concerned, from which he might by proper hypothetical treatment obtain the required law of connexion. But in reality it is seldom possible to carry out this direction except in an approximate manner. Before then we proceed to the consideration of the actual process of quantitative induction, it is necessary to review the several devices by which the complication of effects can be disentangled. Every phenomenon measured will usually be the sum difference or product of two or more different effects, and these must be in some way analysed and separately measured before we possess the materials for a true inductive treatment.

Illustrations of the Complication of Effects.

It is easy to bring forward a multitude of instances to show that a phenomenon is seldom to be observed simple and alone. A more or less elaborate process of analysis is almost always necessary. Thus if an experimentalist wishes to observe and measure the expansion of a liquid by heat, he places it in a thermometer tube and registers the rise of the column of liquid in the narrow tube. But he cannot heat the liquid without also heating the glass, so that the change observed is really the difference between the expansions of the liquid and the glass. More minute investigation will show the necessity perhaps of allowing for further effects, namely the compression of the liquid or the expansion of the bulb due to the increased pressure of the column as it becomes lengthened.

In a great many cases an observed effect will be apparently at least the simple sum of two separate and independent effects. The heat evolved in the combustion of oil is partly due to the carbon and partly to the hydrogen. A measurement of the heat yielded by the two jointly, cannot inform us how much proceeds from the one and how much from the other. If by some separate determination we can ascertain how much the hydrogen yields, then by mere subtraction we learn what is due to the carbon; and *vice versâ*. The heat conveyed by a liquid, may be partly conveyed by true conduction, partly by convection. The light dispersed in the interior of a liquid consists both of what is reflected by floating particles and what is due to true fluorescence[a]; and we must find some mode of determining one portion before we can learn the other.

[a] Stokes, 'Philosophical Transactions' (1852), vol. cxlii. p. 529.

The apparent motion of the spots on the sun, is the algebraic sum of the sun's axial rotation, and of the proper motion of the spots upon the sun's face; hence the difficulty of ascertaining by direct observations the period of the sun's rotation.

We cannot obtain the weight of a portion of liquid in a chemical balance without weighing it with the containing vessel. Hence to have the real weight of the liquid operated upon in an experiment, we must have a separate weighing of the vessel, with or without the adhering film of liquid according to circumstances. This is likewise the mode in which a cart and its load are weighed together, the tare or weight of the cart previously ascertained being deducted. The variation in the height of the barometer is a joint effect, partly due to the real variation of the atmospheric pressure, partly due to the expansion of the mercurial column by heat. The effects may be discriminated, if, instead of one barometer tube we have two tubes placed closely side by side, so as to have exactly the same temperature. If one of them be closed at the bottom so as to be unaffected by the atmospheric pressure, it will show the changes due to temperature only, and, by subtracting these changes from those shown in the other tube, we get the real oscillations of atmospheric pressure. But this correction, as it is called, of the barometric reading, is better effected by calculation from the readings of an ordinary thermometer.

In a great many other cases a quantitative effect will be the difference of two causes acting in opposite directions. The late Sir John Herschel invented an instrument like a large thermometer which he called the Actinometer[b], and M. Pouillet constructed a somewhat similar instrument called the Pyrheliometer, for ascertaining the heating power of the sun's rays. In both instruments the heat of the sun was absorbed by a reservoir containing water, and the rise of temperature of the water was exactly observed, either by its own expansion or by the readings of a delicate thermometer immersed in it. The details of the construction and use of these instruments are immaterial to our immediate purpose. Now in exposing the actinometer to the sun, we do not obtain the full effect of the heat absorbed, because the receiving surface is at the same time radiating heat into empty space. The observed increment of temperature is in short the difference between what is received from the sun and lost by radiation. But the latter quantity is capable of ready determination; we have only to shade the instrument from the direct rays of the sun, while leaving it exposed to the rest of the open sky, and we can observe how much it cools in a certain time. The total effect of the sun's rays will obviously be the apparent effect *plus* the cooling effect in an equal time. By alternate exposure in sun and shade during equal intervals the desired result may be obtained with considerable accuracy[c].

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[b] 'Admiralty Manual of Scientific Enquiry,' edited by Sir John Herschel, 2nd ed. p. 299.

Two quantitative effects were beautifully distinguished in an experiment of John Canton, devised in 1761 for the purpose of demonstrating the compressibility of water[d]. He constructed a thermometer with a large bulb full of water and a short capillary tube, the part of which above the water was freed from air. Under these circumstances the water was relieved from the pressure of the atmosphere, but the glass bulb in bearing that pressure was somewhat contracted. He next placed the instrument under the receiver of an airpump, and on exhausting the air, observed the water sink in the tube. Having thus obtained a measure of the effect of atmospheric pressure on the bulb, he opened the top of the thermometer tube and admitted the air. The level of the water now sank still more, partly from the pressure on the bulb being now compensated, and partly from the compression of the water by the atmospheric pressure. It is obvious that the amount of the latter effect was approximately the difference of the two observed depressions.

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[c] Pouillet, 'Taylor's Scientific Memoirs,' vol. iv. p. 45.

[d] Jamin, 'Cours de Physique,' vol. i. p. 158.

Not uncommonly indeed the actual phenomenon which we wish to measure is considerably less than various disturbing effects which enter into the question. Thus the compressibility of mercury is considerably less than the expansion of the vessels in which it is measured under pressure, so that the attention of the experimentalist has chiefly to be concentrated on the change of magnitude of the vessels. Many astronomical phenomena, such as the parallax or proper motions of the fixed stars, are far less than the instrumental imperfections, and the other phenomena of precession, nutation, aberration, &c. Even Flamsteed imagined he had discovered the parallax of the pole star[e], and time after time astronomers mistook various other phenomena for that minute motion which they were so desirous to discover.

Methods of Eliminating Error.

In any particular experiment it is the object of the experimentalist to measure a single effect only, and he endeavours to obtain that effect free from any interfering effects. If this cannot be, as it seldom or never can really be, he makes the effect as considerable as possible compared with the other effects, which he reduces to a minimum, and treats as noxious errors. Those quantities, which are called *errors* in one case, may really be most important and interesting phenomena in another investigation. When we speak of eliminating error we really mean disentangling the complicated phenomena of nature. The physicist rightly wishes to treat one thing at a time, but as this object can seldom be rigorously carried into practice, he has to seek some mode of counteracting the tendency to error.

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[e] Baily's 'Account of the Rev. John Flamsteed,' p. 58.

The general principle of the subject is that a single observation can render known only a single quantity. Hence if several different quantitative effects are known to enter into any investigation, we must have at least as many distinct results of observation as there are quantities to be determined. Every complete experiment will therefore consist in general of several operations. Guided if possible by previous knowledge of the causes in action, we must arrange these determinations, so that by a simple mathematical process we may distinguish the separate quantities. There appear to be five principal methods in which we may accomplish this object; these methods are specified below and illustrated in the succeeding sections.

(1) *The Method of Avoidance.* The physicist may seek for some special mode of experiment or opportunity of observation, in which the error is non-existent or inappreciable.

(2) *The Differential Method.* He may find opportunities of observation when all interfering phenomena remain constant, and only the subject of observation is at one time present and another time absent; the difference between two exact observations then gives its amount.

(3) *The Method of Correction.* He may endeavour to estimate the amount of the interfering force by the best available mode, and then make a corresponding correction in the results of observation.

(4) *The Method of Compensation.* He may invent some mode of neutralizing the interfering force by balancing against it an exactly equal and opposite force of unknown amount.

(5) *The Method of Reversal.* He may so conduct the experiment that the interfering force may act in opposite directions, in alternate observations, the mean result being free from interference.

1. *Method of Avoidance of Error.*

Astronomers always seek opportunities of observation when errors will have the smallest effect. In spite of elaborate observations and long continued theoretical investigation, it is not found possible to assign any satisfactory law to the refractive power of the atmosphere. Although the apparent change of place of a heavenly body thus produced, may be more or less accurately calculated, yet the error depends upon the temperature and pressure of the atmosphere, and, when a ray is highly inclined to the perpendicular, the uncertainty in the refraction becomes very considerable. Hence astronomers always make their observations, if possible, when the object is at the highest point of its daily course, i.e. on the meridian. In some kinds of investigation, as, for instance, in the determination of the latitude of an observatory, the astronomer is at liberty to select one or more stars out of the countless number visible. There is an evident advantage in such a case, in selecting a star which passes close to the zenith, so that it may be observed almost entirely free from atmospheric refraction, as was done by Hooke. It was ingeniously suggested by Wallis that the parallax of the fixed stars might perhaps be detected by observations of the greatest azimuth east and west of some circumpolar star, since the refractive power of the atmosphere which affects only the altitude would thus be entirely avoided[f].

Astronomers also endeavour to render their clocks as accurate as possible, by removing the source of variation. The pendulum is perfectly isochronous so long as its length remains invariable, and the vibrations are exactly of equal length. They render it nearly invariable in length, that is in the distance between the centres of suspension and oscillation, by a compensatory arrangement for the change of temperature. But as this compensation may not be perfectly accomplished, some astronomers place their chief controlling clocks in a cellar, or other apartment, where the changes of temperature may be as slight as possible. At the Paris Observatory a clock has been placed in the caves beneath the building, where there is no appreciable difference between the summer and winter temperature.

To avoid the effect of unequal oscillations Huyghens made his beautiful investigations, which resulted in the discovery that a pendulum, of which the centre of oscillation moved upon a cycloidal path, would be perfectly isochronous, whatever the variation in the length of oscillations. But though a pendulum may be rendered in some degree cycloidal by the use of a steel suspension spring, it is found that the mechanical arrangements requisite to produce a truly cycloidal motion introduce more error than they avoid. Hence astronomers seek to reduce the error to the smallest amount by maintaining their clock pendulums in uniform movement[g]; and in fact while a clock is in good order and has the same weights, there need be little change in the length of oscillation.

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[f] Grant, 'History of Physical Astronomy,' p. 548.

[g] Montucla, 'Histoire des Mathématiques,' vol. ii. p. 420.

When a pendulum cannot be made to swing uniformly, as in experiments upon the force of gravity, it becomes requisite to resort to the third method, and a correction is introduced, calculated on theoretical grounds from the amount of the observed change in the length of vibration.

It has been mentioned that the apparent expansion of a liquid by heat, when contained in a thermometer tube or other vessel, is the difference between the real expansion of the liquid and that of the containing vessel. The effects can be accurately distinguished provided that we can learn the real expansion by heat of any one convenient liquid; for by observing the apparent expansion of the same liquid in any required vessel we can by difference learn the amount of expansion of the vessel due to any given change of temperature. When we once know the change of dimensions of the vessel, we can of course determine the absolute expansion of any other liquid tested in it. Thus it became an all-important object in scientific research to measure with accuracy the absolute dilatation by heat of some one liquid, and mercury owing to several circumstances was by far the most suitable. Dulong and Petit devised a beautiful mode of effecting this by simply avoiding altogether the effect of the change of size of the vessel. Two upright tubes full of mercury were connected by a fine tube at the bottom, and were maintained at two different temperatures. As mercury was free to flow from one tube to the other by the connecting tube, the two columns necessarily exerted equal pressures by the principles of hydrostatics. Hence it was only necessary to measure very accurately by a cathetometer the difference of level of the surfaces of the two columns of mercury, to learn the difference of length of columns of equal hydrostatic pressure, which at once gives the difference of density of the mercury, and the dilatation by heat. The changes of dimension in the containing tubes now became a matter of entire indifference, and the length of a column of mercury at different temperatures was measured as easily as if it had formed a solid bar. The experiment was carried out by Regnault with many improvements of detail, and the absolute dilatation of mercury, at temperatures between 0° Cent. and 350°, was determined almost as accurately as was needful[h].

The presence of a large and uncertain amount of error may often render a method of experiment valueless. Foucault's beautiful mode of demonstrating the rotation of the earth by the motion of a pendulum was thus frustrated. The slightest lateral disturbance of the pendulum gave it an elliptical path with a progressive motion of the axis of the ellipse, and this motion of an unknown amount disguised and overpowered that due to the rotation of the earth[i]. Faraday's laborious experiments on the relation of gravity and electricity were much obstructed, too, by the fact that it is almost impossible to move a large weight of iron or even lead without generating currents of electricity, either by friction or induction. To distinguish the electricity directly due to the action of gravity from the greater quantities indirectly produced would have been a problem of excessive difficulty. Baily in his experiments on the density of the earth was aware of the existence of inexplicable disturbances which have since been referred to the action of electricity with much probability[k]. The skill and ingenuity of the experimentalist are often exhausted in devising a form of apparatus in which such causes of error shall be reduced to a minimum.

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[h] Jamin, 'Cours de Physique,' vol. ii. pp. 15–28.

[i] 'Philosophical Magazine,' 1851, 4th Series, vol. ii. *passim*.

[k] Hearn, 'Philosophical Transactions,' 1847, vol. cxxxvii. pp. 217–221.

In some rudimentary experiments we may wish merely to establish the existence of a quantitative effect without precisely measuring its amount; if there exist causes of error of which we can neither render the amount known or inappreciable, the best way will be to make them all negative so that the quantitative effects will be less than the truth rather than greater. Mr. Grove, for instance, in proving that the magnetization or demagnetization of a piece of iron raises its temperature, took care to maintain the electro-magnet by which the iron was acted upon at a lower temperature, so that it would cool rather than warm the iron by radiation or conduction[l].

Rumford's celebrated experiment to prove that heat was generated out of mechanical force in the boring of a cannon was subject to the difficulty that heat might be brought to the cannon by conduction from neighbouring bodies. It was an ingenious device of Davy to produce friction by a piece of clock-work resting upon a block of ice in an exhausted receiver; as the machine rose in temperature above 32°, it was certain that no heat was received by conduction from the support[m]. In many other experiments ice may be employed to prevent the access of heat by conduction, and this device, first put in practice by Murray[n], is beautifully employed in Bunsen's calorimeter.

To obtain the true temperature of the air, though apparently so easy, is really a very difficult matter, because the thermometer employed is sure to be affected either by the sun's rays, the radiation from neighbouring objects, or the escape of heat into space. These sources of error are too fluctuating to allow of correction, so that the only accurate mode of procedure is that devised by Dr. Joule, of surrounding the thermometer with a copper cylinder ingeniously adjusted to the temperature of the air, as described by him, so that the effect of radiation shall be nullified [o].

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[l] 'The Correlation of Physical Forces,' 3rd ed. p. 159.

[m] 'Collected works of Sir H. Davy,' vol. ii. pp. 12–14. 'Elements of Chemical Philosophy,' p. 94.

[n] 'Nicholson's Journal,' vol. i. p. 241; quoted in 'Treatise on Heat,' Useful Knowledge Society, p. 24.

When the avoidance of error cannot be carried into effect, it will yet be desirable to reduce the absolute amount of the interfering error as much as possible before employing the succeeding methods to correct the result. As a general rule we can determine a quantity with less inaccuracy as it is smaller, so that if the error itself be small the error in determining that error will be of a still lower order of magnitude. But in some cases the absolute amount of an error is of no consequence, as in the index error of a divided circle, or the difference between a chronometer and astronomical time. Even the rate at which a clock gains or loses is a matter of little importance provided it remains constant, so that a sure calculation of its amount can be made.

2. *Differential Method.*

When we cannot avoid the entrance of error, we can often resort with great success to the second mode of measuring phenomena under such circumstances that the error shall remain nearly or quite the same in all the observations, and neutralize itself as regards the purposes in view. This mode is available whenever we want a difference between quantities and not the absolute quantity of either. The determination of the parallax of the fixed stars is exceedingly difficult, because the amount of parallax is far less than most of the corrections for atmospheric refraction, nutation, aberration, precession, instrumental irregularities, &c., and can with difficulty be detected among these phenomena of various magnitude. But, as Galileo long ago suggested, all such difficulties would be avoided by the differential observation of stars, which though apparently close together are really far separated on the line of sight. Two such stars in close apparent proximity will be subject to almost exactly equal errors, so that all we need do is to observe the apparent change of place of the nearer star as referred to the more distant one. A good telescope furnished with an accurate micrometer is alone needed for the application of the method. Huyghens appears to have been the first observer who actually tried to employ the method practically[p], but it was not until 1835 that the improvement of telescopes and micrometers enabled Struve to detect in this way the parallax of the star α Lyræ.

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[o] Clerk Maxwell, 'Theory of Heat,' p. 228. 'Proceedings of the Manchester Philosophical Society,' Nov. 26, 1867, vol. vii. p. 35.

It is one of the many advantages of the observation of transits of Venus for the determination of the solar parallax that the refraction of the atmosphere affects in an exactly equal degree the planet and the portion of the sun's face over which it is passing. Thus the observations are strictly of a differential nature.

By the process of substitutive weighing it is possible to ascertain the equality or inequality of two weights with almost perfect freedom from error. If two weights A and B be placed in the scales of the best balance we cannot be sure that the equilibrium of the beam indicates exact equality, because the arms of the beam may be unequal or unbalanced. But if we take B out and put another weight C in, and equilibrium still exists, it is apparent that the same causes of erroneous weighing exist in both cases, supposing that the balance has not been disarranged, and that B must be exactly equal to C, since it has exactly the same effect under the same circumstances. In like manner it is a general rule that, if by any uniform mechanical process we get a copy of an object, it is unlikely that this copy will be precisely the same in magnitude and form as the original, but two copies will equally diverge from the original, and will therefore almost exactly resemble each other.

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[p] History of 'Physical Astronomy,' p. 549. Herschel's 'Outlines of Astronomy,' 4th ed. p. 550.

Leslie's Differential Thermometer[q] was well adapted to the experiments for which it was invented. Having two equal bulbs any alteration in the temperature of the air will act equally by conduction on each and produce no change in the indications of the instrument. Only that radiant heat which is purposely thrown upon one of the bulbs will produce any effect. This thermometer in short carries out the principle of the differential method in a mechanical manner.

3. *Method of Correction.*

Whenever the result of an experiment is affected by an interfering cause to an amount either invariable or exactly calculable, it is sufficient simply to add or subtract this calculated amount. We are said to correct observations when we thus eliminate what is due to extraneous causes, although of course we are only separating the correct effects of several agents. Thus the variation in the height of the barometrical column is partly due to the change of temperature, and since the coefficient of absolute dilatation of mercury has been exactly determined, as already described (p. 395), we have only to make calculations of a simple character, or, what is better still, tabulate a series of such calculations for general use, and the correction for temperature can be made with all desired accuracy. The height of the mercury in the barometer is also affected by capillary attraction, which depresses it by a constant amount depending on the diameter of the tube. The requisite corrections can be estimated with accuracy sufficient for most purposes, more especially as we can check the correctness of the reading of a barometer by comparison with a perfect standard barometer, and introduce if need be an index error including both the error in the affixing of the scale and the effect due to capillarity. But in constructing the standard barometer itself we must take greater precautions; the capillary depression depends somewhat upon the quality of the glass, the absence of air, and the perfect cleanliness of the mercury, so that we cannot with confidence assign the exact amount of the effect. Hence a standard barometer is constructed with a wide tube, sometimes even an inch in diameter, so that the capillary effect may be rendered of little account[r]. Gay Lussac made barometers in the form of a siphon so that the capillary forces acting equally at the upper and lower surfaces should balance and destroy each other, but the method fails in practice because the lower surface, being open to the air, becomes sullied and subject to a different force of capillarity.

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[q] Leslie's 'Inquiry into the Nature of Heat,' p. 10.

In a great many mechanical experiments friction is an interfering condition, and drains away a portion of the energy intended to be operated upon in a definite manner. We should of course reduce the friction in the first place to the lowest possible amount, but as it cannot be altogether prevented, and is not calculable with certainty from any general laws, we must determine it separately for each apparatus by suitable experiments. Thus Smeaton, in his admirable but now almost forgotten researches concerning water-wheels, eliminated friction in the most simple manner by determining by trial what weight, acting by a cord and roller upon his model water-wheel, would make it turn without water as rapidly as the water made it turn. In short, he ascertained what weight concurring with the water would exactly compensate for the friction[s]. In Dr. Joule's experiments to determine the mechanical equivalent of heat by the condensation of air, a considerable amount of heat was produced by friction of the condensing pump, and a small portion by stirring the water employed to measure the heat. This heat of friction was ascertained by simply repeating the experiment in an exactly similar manner except that no condensation was effected, and observing the change of temperature then produced[t].

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[r] Watts' 'Dictionary of Chemistry,' vol. i. pp. 513–15.

We may describe as *test experiments* any in which we perform operations not intended to give the quantity of the principal phenomenon, but some quantity which would otherwise remain as an error in the result. Thus in astronomical observations almost every source of error may be avoided by increasing the number of observations and distributing them in such a manner as to produce in the final mean as much error in one way as in the other. But there is one source of error, first discovered by Maskelyne, which cannot be avoided, because it affects all observations in the same direction and to the same average amount, namely the Personal Error of the observer or the inclination to record the passage of a star across the wires of the telescope a little too soon or a little too late. This personal error was first described in the 'Edinburgh Journal of Science,' vol. i. p. 178. The difference between the judgment of observers at the Greenwich Observatory usually varies from $\frac{1}{100}$ to $\frac{1}{3}$ of a second, or even a little more, and remains pretty constant for the same observers[u]. In some observers it has amounted to seven or eight-tenths of a second[x]. De Morgan appears to have entertained the opinion that this source of error was essentially incapable of elimination or correction[y]. But it seems clear that this personal error might be determined absolutely with any desirable degree of accuracy by test experiments, consisting in making an artificial star move at a considerable distance and recording by electricity the exact moment of its passage over the wire. This method has in fact been successfully employed in Leyden, Paris, and Neuchatel[z].

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[s] 'Philosophical Transactions,' vol. li. p. 100.

[t] 'Philosophical Magazine,' 3rd Series, vol. xxvi. p. 372.

Newton employed the pendulum for making experiments on the impact of balls. Two balls were hung in contact, and one of them, being drawn aside through a measured arc, was then allowed to strike the other, the arcs of vibration giving sufficient data for calculating the distribution of energy at the moment of impact. The resistance of the air was an interfering cause which he estimated very simply by causing one of the balls to make several complete vibrations and then marking the reduction in the length of the arcs, a proper fraction of which reduction was added to each of the other observed arcs of vibration[a].

In the modern use of the pendulum, to measure terrestrial gravity, it is not found convenient to annul the resistance of the air by operating in a vacuum. Consequently this resistance has to be ascertained by appropriate and tedious series of experiments, which should be made if possible upon each pendulum employed.

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[u] 'Greenwich Observations for 1866,' p. xlix.

[x] 'Penny Cyclopædia,' art. *Transit*, vol. xxv. pp. 129, 130.

[y] Ibid. art. *Observation*, p. 390.

[z] 'Nature,' vol. i. pp. 85, 337. See references to the Memoirs describing the method.

[a] 'Principia,' Book I. Law III. Corollary VI. Scholium. Motte's translation, vol. i. p. 33.

The exact definition of the standard of length is one of the most important, as it is one of the most difficult questions in physical science, and the different practice of different nations introduces wholly needless confusion. Were all standards constructed so as to give the true length at a fixed uniform temperature, for instance the freezing-point, then any two standards could be compared without the interference of temperature by bringing them both to exactly the same fixed temperature. Unfortunately the French metre is defined by a bar of platinum at 0°C, while our yard is defined by a bronze bar at 62°F. It is quite impossible, then, to make a comparison of the yard and metre without the introduction of a correction, either for the expansion of platinum or bronze, or both. Bars of metal differ too so much in their rates of expansion according to their molecular condition that it is dangerous to infer from one bar to another.

When we come to use instruments with great accuracy there are many minute sources of error which must be guarded against. If a thermometer has been graduated when perpendicular, it will read somewhat differently when laid down, as the pressure of a column of mercury is removed from the bulb. The reading may also be somewhat altered if it has recently been raised to a higher temperature than usual, if it be placed under a vacuous receiver, or if the tube be unequally heated as compared with the bulb. For these minute causes of error we may have to introduce troublesome corrections, unless we adopt the simple mode of using the thermometer in circumstances of position, &c. exactly similar to those in which it was graduated[c]. There is no end to the number of minute corrections which may ultimately be required. A very large number of experiments on gases, standard weights and measures, &c. depend upon the height of the barometer; but when experiments in different parts of the world are compared together we ought to take into account the varying force of gravity, which even between London and Paris makes a difference of ·008 inch of mercury.

The measurement of quantities of heat is a matter of great difficulty, because there is no known substance impervious to heat, and the problem is therefore as difficult as to measure liquids in porous vessels. To determine the latent heat of steam we must condense a certain amount of the steam in a known weight of water, and then observe the rise of temperature of the water. But while we are carrying out the experiment, part of the heat will have escaped by radiation or conduction from the condensing vessel or calorimeter. We may indeed reduce the loss of heat by using vessels with double sides and bright surfaces, surrounded with swan's-down wool or other non-conducting materials; and we may also avoid raising the temperature of the water much above that of the surrounding air. Yet we cannot by any such means render the loss of heat inconsiderable. Rumford ingeniously proposed to reduce the loss to zero by commencing the experiment when the temperature of the calorimeter is as much below that of the air as it is at the end of the experiment above it. Thus the vessel will first gain and then lose by radiation and conduction, and these opposite errors will approximately balance each other. But Regnault has shown that the loss and gain do not proceed by exactly the same laws, so that in very accurate investigations Rumford's method is not sufficient. There remains the method of correction which was beautifully carried out by Regnault in his determination of the latent heat of steam. He employed two calorimeters, made in exactly the same way and alternately used to condense a certain amount of steam, so that while one was measuring the latent heat, the other calorimeter was engaged in determining the corrections to be applied, whether on account of radiation and conduction from the vessel or on account of heat reaching the vessel by means of the connecting pipes[d].

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[c] Balfour Stewart, 'Elementary Treatise on Heat,' p. 16.

4. *Method of Compensation.*

There are many cases in which a cause of error cannot conveniently be rendered null, and is yet beyond the reach of the third method, that of calculating the requisite correction from independent observations. The magnitude of an error may be subject to continual variations, on account of change of weather, or other fickle circumstances beyond our control. It may either be impracticable to observe the variation of those circumstances in sufficient detail, or, if observed, the calculation of the amount of error may be subject to doubt. In these cases, and only in these cases, it will be desirable to invent some artificial mode of counterpoising the variable error against an equal error subject to exactly the same variation.

We cannot weigh any object with great accuracy unless we make a correction for the weight of the air displaced by the object, and add this to the apparent weight. In very accurate investigations relating to standard weights, it is usual to note the barometer and thermometer at the time of making a weighing, and, from the measured bulks of the objects compared, to calculate the weight of air displaced; the third method in fact is adopted. To make all the calculations in the frequent weighings requisite in chemical analysis would be exceedingly laborious, hence the correction is usually neglected. But when the chemist wishes to weigh a quantity of gas contained in a glass globe for the purpose of determining its specific gravity, the correction becomes of much importance. Hence chemists avoid at once the error, and the labour of correcting it, by attaching to the opposite scale of the balance a sealed glass globe of exactly equal capacity to that containing the gas to be weighed, noting only the difference of weight when the globe is full and empty. The correction, being exactly the same for both globes, may be entirely neglected[e].

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[d] Graham's 'Chemical Reports and Memoirs,' Cavendish Society, pp. 247, 268, &c.

A device of nearly the same kind is employed in the construction of galvanometers which measure the force of an electric current by the deflection of a suspended magnetic needle. The resistance of the needle is partly due to the directive influence of the earth's magnetism, and partly to the torsion of the thread. But the former force may often be inconveniently great as well as troublesome to determine for different inclinations. Hence it is customary to connect together two exactly equal needles, with their poles pointing in opposite directions, one needle being within and another without the coil of wire. As regards the earth's magnetism, the needles are now *astatic* or indifferent, the tendency of one needle being exactly balanced by that of the other.

An elegant instance of the elimination of a disturbing force by compensation is found in Faraday's researches upon the magnetism of gases. To observe the magnetic attraction or repulsion of a gas seems impossible unless we enclose the gas in an envelope, probably best made of glass. But any such envelope is sure to be more or less affected by the magnet, so that it becomes difficult to distinguish between three forces which enter the problem; namely, the magnetism of the gas in question, that of the envelope, and that of the surrounding atmospheric air. Faraday avoided all difficulties by employing two exactly equal and similar glass tubes connected together, and so suspended from the arm of a torsion balance that the tubes were in similar parts of the magnetic field. One tube being filled with nitrogen and the other with oxygen, it was found that the oxygen seemed to be attracted and the nitrogen repelled. The suspending thread of the balance was then turned until the force of torsion restored the tubes to their original places, where the magnetism of the tubes as well as that of the surrounding air, being exactly the same and in the opposite direction upon the two tubes, could not produce any interference. The force thus required to restore the tubes was measured by the amount of torsion of the thread, and it indicated correctly the comparative attractive powers of oxygen and nitrogen. The oxygen was then withdrawn from one of the tubes, and a second experiment made, so as to compare a vacuum with nitrogen. No force was now required to maintain the tubes in their places, so that nitrogen was found to be, approximately speaking, indifferent to the magnet, that is, neither magnetic nor diamagnetic, while oxygen was proved to be positively magnetic[f]. It required the highest experimental skill on the part of Faraday and Tyndall, to distinguish between what is apparent and real in magnetic attraction and repulsion.

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[e] Regnault's 'Cours Elémentaire de Chimie,' 1851, vol. I. p. 141.

Experience alone can absolutely decide when a compensating arrangement is conducive to accuracy. As a general rule mechanical compensation is the last resource, and in the more accurate observations it is likely to introduce more uncertainty than it removes. A multitude of instruments involving mechanical compensation have been devised, but they are usually of an unscientific character[g], because the errors compensated can be more accurately determined and allowed for. But there are exceptions to this rule, and it seems to be proved that in the delicate and tiresome operation of measuring a base line, invariable bars, compensated for expansion by heat, give a very accurate result, the observation of their varying temperature and the calculation of the corrections being an uncertain and tedious work[h].

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[f] Tyndall's 'Faraday,' pp. 114–15.

We thus see that the choice of one or other mode of eliminating a simple error depends entirely upon circumstances and the object in view; but we may safely lay down the following conclusions. First of all, seek to avoid the source of error altogether if it can be conveniently done; if not, make the experiment so that the error may be as small, but more especially as constant, as possible. If the means are at hand for determining its amount by calculation from other experiments and principles of science, allow the error to exist and make a correction in the result. If this cannot be accurately done or involves too much labour for the purposes in view, then throw in a counteracting error which shall as nearly as possible be of equal amount in all circumstances with that to be eliminated. There yet remains, however, one important method, that of Reversal, which will form an appropriate transition to the succeeding chapters on the Method of Mean Results and the Law of Error.

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[g] See, for instance, the Compensated Sympiesometer, 'Philosophical Magazine,' 4th Series, vol. xxxix. p. 371.

[h] Grant, 'History of Physical Astronomy,' pp. 146, 147.

5. *Method of Reversal.*

The fifth method of eliminating error is most potent and satisfactory whenever it can be applied, but it requires that we shall be able to reverse the apparatus and mode of procedure, so as to make the interfering cause act alternately in opposite directions. If we can get two experimental results, one of which is as much too great as the other is too small, the error is equal to half the difference, and the true result is the mean of the two apparent results. It is an unavoidable defect of the chemical balance, for instance, that the points of suspension of the pans cannot be fixed at exactly equal distances from the centre of suspension of the beam. Hence two weights which seem to balance each other will never be quite equal in reality. The difference is detected by reversing the weights, and it may be estimated by adding sufficient small weights to the deficient side to restore equilibrium, and then taking as the true weight the geometric mean of the two apparent weights of the same object. If the difference is small the arithmetic mean, that is half the sum, may be substituted for the geometric mean, from which it will not appreciably differ.

This method of reversal is most extensively employed in practical astronomy. The apparent elevation of a heavenly body is observed by a telescope moving upon a divided circle, upon which the inclination of the telescope is read off. Now this reading will be erroneous if the circle and the telescope have not accurately the same centre. But if we read off at the same time both ends of the telescope, the one reading will be about as much too small as the other is too great, and the mean will be nearly free from error. In practice the observation is differently conducted, but the principle is the same; the telescope is fixed to the circle, which moves with it, and the angle through which it moves is read off at three, six, or more points, disposed of at equal intervals round the circle. The older astronomers, down even to the time of Flamsteed, were accustomed to use portions only of a divided circle, generally quadrants, and Römer made a vast improvement when he introduced the complete circle.

The transit circle, employed to determine the meridian passage of heavenly bodies, is so constructed that the telescope and the axis bearing it, in fact the whole moving part of the instrument, can be taken out of the bearing sockets and turned over, so that what was formerly the western pivot becomes the eastern one, and *vice versâ.* It is impossible that the instrument could have been so perfectly constructed, mounted, and adjusted that the telescope should point exactly to the meridian, but the effect of the reversal is that it will point as much to the west in one position as it does to the east in the other, and the mean result of observations in the two positions must be free from such cause of error.

The accuracy with which the inclination of the compass needle can be determined depends almost entirely on the method of reversal. The dip needle consists of a bar of magnetized steel, suspended like the beam of a delicate balance on a slender axis passing through the centre of gravity of the bar, so that it is at liberty to rest in that exact degree of inclination in the magnetic meridian which the magnetism of the earth induces. The inclination is read off upon a vertical divided circle, but to avoid any error in the centring of the needle and circle, both ends are read, and the mean of the results is taken. The whole instrument is now turned carefully round through 180°, which gives two new readings, in which any error due to the wrong position of the zero of the division will be reversed. As the axis of the needle may not be exactly horizontal, it is now reversed in the same manner as the transit instrument, the end of the axis which formerly pointed east being made to point west, and a new set of readings is taken.

Finally, error may arise from the axis not passing accurately through the centre of gravity of the bar, and this error can only be detected and eliminated on reversing the magnetic poles of the bar by the application of a strong magnet. The error is thus made to act in opposite directions. To ensure all possible accuracy each reversal ought to be combined with each other reversal, so that the needle will be observed in eight different positions by sixteen different readings, the mean of the whole of which will give the required inclination free from all eliminable errors[k].

There are certain cases of experiment in which a disturbing cause can with much ease be made to act in opposite directions, in alternate observations, so that the mean of the results will be free from disturbance. Thus in direct experiments upon the velocity of sound in passing through the air between stations two or three miles apart, the wind is a cause of error. It will be well, in the first place, to choose a time for the experiment when the air is very nearly at rest, and the disturbance slight, but if at the same moment signal sounds be made at each station and observed at the other, two sounds will be passing in opposite directions through the same body of air and the wind will accelerate one sound almost exactly as much as it retards the other[l]. Again, in trigonometrical surveys the apparent height of a point will be affected by atmospheric refraction and the curvature of the earth. But if in the case of two points the apparent elevation of each as seen from the other be observed, the corrections will be the same in amount, but reversed in direction, and the mean between the two apparent differences of altitude will give the true difference of level[m].

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[k] Quetelet, 'Sur la Physique du Globe,' p. 174. Jamin, 'Cours de Physique,' vol. i. p. 504.

[l] Herschel, On Sound, 'Encyclopædia Metropolitana,' p. 748.

In the next two chapters we really pursue the Method of Reversal into more complicated applications.

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[m] Hutton, 'Philosophical Transactions,' abridgment, vol. xiv. p. 422.

CHAPTER XVI.

THE METHOD OF MEANS.

ALL results of the measurement of continuous quantity can only be approximately true. Were this assertion doubted, it could readily be proved by direct experience. For if any person, using an instrument of the greatest precision, makes and registers successive observations in an unbiassed manner, it will almost invariably be found that the results differ from each other. When we operate with sufficient care we cannot perform so simple an experiment as weighing an object in a good balance without getting discrepant numbers. Only the rough and careless experimenter will think that his observations agree, but in reality he will be found to overlook the differences. The most elaborate researches, such as those undertaken in connexion with standard weights and measures, always render it apparent that complete coincidence is out of the question, and that the more accurate our modes of observation are rendered, the more numerous are the sources of minute error which become apparent. We may look upon the existence of error in all measurements as the normal state of things. It is absolutely impossible to eliminate separately the multitude of small disturbing influences, except by balancing them off against each other. And even in drawing a mean it is to be expected that we shall come near the truth rather than exactly to it. In the measurement of continuous quantity, absolute coincidence, if it even occurs or seems to occur, must be purely casual, and is no indication of precision. It is one of the most embarrassing things we can meet when experimental results agree too closely. Such coincidences should raise our suspicion that the apparatus in use is in some way restricted in its operation, so as not really to give the true result at all[a], or that the actual results have not been faithfully recorded by the assistant in charge of the apparatus.

If then we cannot get twice over exactly the same result, the question arises, How can we ever attain the truth or select the result which may be supposed to approach most nearly to it? The quantity of a certain phenomenon is expressed in several numbers which differ from each other; no more than one of them at the most can be true, and it is more probable that they are all false. It may be suggested, perhaps, that the observer should select the one observation which he judged to be the best made, and there will often doubtless be a feeling that one or more results were satisfactory, and the others less trustworthy. This seems to have been the course adopted by some of the early astronomers. Flamsteed when he had made several observations of a star probably chose in an arbitrary manner that which seemed to him nearest to the truth[b].

When Horrocks selects for his estimate of the sun's semidiameter a mean between the results of Kepler and Tycho he professes not to do it from any regard to the idle adage, 'Medio tutissimus ibis,' but because he thought it from his own observations to be correct[c]. But this method will not apply at all when the observer has made a number of measurements which are equally good in his opinion, and it is quite apparent that in using an instrument or apparatus of considerable complication the observer will not necessarily be able to judge whether slight causes have affected its operation or not.

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[a] Thomson and Tait, 'Treatise on Natural Philosophy,' vol. i. p. 309.

[b] Baily's 'Account of Flamsteed,' p. 376.

[c] 'The Transit of Venus across the Sun,' by Horrocks, London, 1859, p. 146.

In this question, as indeed throughout inductive logic, we deal only with probabilities. There is no infallible mode of arriving at the absolute truth, which lies beyond the reach of human intellect, and can only be the distant object of our long continued and painful approximations. Nevertheless there is a mode pointed out alike by common sense and the highest mathematical reasoning, which is more likely than any other, as a general rule, to bring us near the truth. The ἄριστον μέτρον, or the *aurea mediocritas*, was highly esteemed in the ancient philosophy of Greece and Rome; but it is not probable that any of the ancients should have been able clearly to analyse and express the reasons why they advocated the *mean* as the safest course. But in the last two centuries this apparently simple question of the mean has been found to afford a field for the exercise of the utmost mathematical skill. Roger Cotes, the editor of the 'Principia,' appears to have had some insight into the value of the mean; but profound mathematicians such as De Moivre, Daniel Bernouilli, Laplace, Lagrange, Gauss, Quetelet, De Morgan, Airy, Leslie, Ellis and others have hardly exhausted the subject.

Several uses of the Mean Result.

The elimination of errors of unknown sources, is almost always accomplished by the simple arithmetical process of taking the *mean*, or, as it is often called, the *average* of several discrepant numbers. To take an average is to add the several quantities together, and divide by the number of quantities thus added, which gives a quotient lying among, or in the *middle* of, the several quantities. Before however inquiring fully into the grounds of this procedure, it is essential to observe that this one arithmetical process is really applied in at least three different cases, for different purposes, and upon different principles, and we must take great care not to confuse one application of the process with another. A *mean result*, then, may have any one of the following significations.

(1) It may give a merely representative number, expressing the general magnitude of a series of quantities, and serving as a convenient mode of comparing them with other series of quantities. Such a number is properly called *The fictitious mean* or *The average result.*

(2) It may give a result approximately free from disturbing quantities, which are known to affect some results in one direction, and other results equally in the opposite direction. We may say that in this case we get a *Precise mean result.*

(3) It may give a result more or less free from unknown and uncertain errors; this we may call the *Probable mean result.*

Of these three uses of the mean the first is entirely different in nature from the two last, since it does not yield an approximation to any natural quantity, but furnishes us with an arithmetic result comparing the aggregate of certain quantities with their number. The third use of the mean rests entirely upon the theory of probability, and will be more fully considered in a later part of this chapter. The second use is closely connected, or even identical with, the Method of Reversal already described (p. 410), but it will be convenient to enter somewhat fully on all the three employments of the same arithmetical process.

The significations of the terms Mean and Average.

Much confusion exists in the popular, or even the scientific employment of the terms *mean* and *average*, and they are commonly taken as synonymous. It is desirable to ascertain carefully what significations we ought to attach to them. The English word *mean* is exactly equivalent to *medium*, being derived perhaps, through the French *moyen*, from the latin *medius*, which again is undoubtedly kindred with the Greek μεσος. Etymologists believe, too, that this Greek word is connected with the preposition μετα, the German *mitte*, and the true English *mid* or *middle*; so that after all the *mean* is a technical term identical in its root with the more popular equivalent *middle*.

If we inquire what is a mean in a mathematical point of view, the true answer is that there are several or many kinds of means. The old arithmeticians recognised at least ten kinds, which are stated by Boethius, and even an eleventh was added by Jordanus[d].

The *arithmetic mean* is the one by far the most commonly denoted by the term, and that which we may understand it to signify in the absence of any qualification. It is the sum of any series of quantities divided by their number, and may be represented by the formula $\frac{1}{2}(a+b)$. But there is also the *geometric mean*, which is the square root of the product, $\sqrt{a \times b}$, or that quantity the logarithm of which is the arithmetic mean of the logarithms of the quantities. There is also the *harmonic mean*, which is the reciprocal of the arithmetic mean of the reciprocals of the quantities. Thus if a and b be the quantities, as before, their reciprocals are $\frac{1}{a}$ and $\frac{1}{b}$, the mean of which is $\frac{1}{2}\left(\frac{1}{a}+\frac{1}{b}\right)$, and the reciprocal again is $\frac{2ab}{a+b}$. Other kinds of means might no doubt be invented for particular purposes, and we might apply the term, as De Morgan pointed out[e], to any quantity a function of which is equal to a function of two or more other quantities, and is such, that the interchange of these latter quantities among themselves will make no alteration in the value of the function. Symbolically, if $\phi(y, y, y \ldots.) = \phi(x_1, x_2, x_3 \ldots.)$, then y is a kind of mean of the quantities x_1, x_2, &c.

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[d] De Morgan, Supplement to the 'Penny Cyclopædia,' art. *Old Appellations of Numbers.*

The geometric mean is necessarily adopted in certain cases. Thus when we estimate the work done against a force which varies inversely as the square of the distance from a fixed point, the mean force is the geometric mean between the forces at the beginning and end of the path[f]. When in an imperfect balance, we reverse the weights to eliminate error, the true weight will be the geometric mean of the two apparent weights of the one body (see p. 410).

In almost all the calculations of statistics and commerce the geometric mean ought, strictly speaking, to be used. Thus if a commodity rises in price 100 per cent. and another remains unaltered, the mean rise of price is not 50 per cent. because the ratio 150 : 200 is not the same as 100 : 150. The mean ratio is as unity to $\sqrt{1\cdot00 \times 2\cdot00}$ or 1 to 1·41. The difference between the three kinds of mean in such a case, as I have elsewhere shown[g], is very considerable, being as follows—

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[e] 'Penny Cyclopædia,' art. *Mean*.

[f] Thomson and Tait, 'Treatise on Natural Philosophy,' vol. i. p. 366.

[g] 'Journal of the Statistical Society,' June 865, vol. xxviii. p. 296.

Arithmetic mean 50 per cent.
Geometric „ 41 „
Harmonic „ 33 „

In all calculations concerning the average rate of progress of a community, or any of its operations, the geometric mean should be employed. For if a quantity increases 100 per cent. in 100 years, it would not on the average increase 10 per cent. in each ten years, as the 10 per cent. would at the end of each decade be calculated upon larger and larger quantities, and give at the end of 100 years much more than 100 per cent., in fact as much as 159 per cent. The true mean rate in each decade would be $\sqrt[10]{2}$ or about 1·07, that is, the increase would be about 7 per cent. in each ten years. But when the quantities differ but little, the arithmetic and geometric means are approximately the same. Thus the arithmetic mean of 1·000 and 1·001 is 1·0005, and the geometric mean is about 1·0004998, the difference being of an order inappreciable in almost all scientific or practical processes. Even in the comparison of standard weights by Gauss' method of transposition the arithmetic mean may usually be substituted for the geometric mean which is the true result.

Regarding the mean in the absence of express qualification to the contrary as the common arithmetic mean, we must still distinguish between its two uses where it defines with more or less accuracy and probability a really existing quantity, and where it acts as a mere representative of other quantities. If I make many experiments to determine the specific gravity of a homogeneous piece of gold there is a certain definite ratio which I wish to approximate to, and the mean of my separate results will, in the absence of any reasons to the contrary, be the most probable approximate result. When we determine on the other hand the mean density of the earth, it is exceedingly unlikely that there is any part of the earth exactly of that density, and, as the crust is only about half the mean density, there must be other parts of greater density. I may also determine the mean specific gravity of a body composed of iron and gold, so that there will certainly be no portion possessing the mean density.

The very different signification of the word 'mean' in these two uses has been fully explained by M. Quetelet[h], and the importance of the distinction has moreover been pointed out by Sir John Herschel in reviewing his work[i]. It is much to be desired that scientific men would mark the difference by using the word *mean* only in the former sense when it denotes approximation to a definite existing quantity; and *average*, when the mean is only a fictitious quantity, used for the convenience of thought and expression. The etymology of this word 'average' is somewhat obscure; but according to De Morgan[k] it comes from *averia*, 'havings or possessions,' especially applied to farm stock. By the accidents of language *averagium* came to mean the labour of farm horses to which the lord was entitled, and it probably acquired in this manner the notion of distributing a whole into parts, a sense in which it was very early applied to maritime averages or contributions of the other owners of cargo to those whose goods have been thrown overboard or used for the safety of the vessel.

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[h] 'Letters on the Theory of Probabilities,' transl. by Downes, Part ii.

[i] Herschel's 'Essays,' &c. pp. 404, 405.

[k] 'On the Theory of Errors of Observations,' 'Cambridge Philosophical Transactions,' vol. x. Part ii. 416.

On the Fictitious Mean or Average Result.

Although the average when employed in its proper sense of a fictitious mean, represents no really existing quantity, it is yet of the highest scientific importance, as enabling us to conceive in a single result a multitude of complex details. It enables us to make a hypothetical simplification of a problem, and avoid complexity without committing error. Thus the aggregate weight of a body is the sum of the weights of the indefinitely small particles, each acting at a different place, so that the simplest mechanical problem concerning a body really resolves itself, strictly speaking, into an infinite number of distinct problems. We owe to Archimedes the first introduction of the beautiful idea that one point might be discovered in a gravitating body such that the weight of all the particles might be regarded as concentrated in that point, and yet the behaviour of the whole body would be exactly represented by the behaviour of this heavy point. This Centre of Gravity may be within the body, as in the case of a sphere, or it may be in empty space, as in the case of a ring. Any two bodies, whether connected or separate, may be conceived as having a centre of gravity; that of the sun and earth, for instance, lying within the sun and only 267 miles from its centre.

Although we most commonly use the notion of a centre or average point with regard to gravity, the same notion is applicable to many other cases. Terrestrial gravity is only one case of approximately parallel forces, so that the centre of gravity is but a special case of the more general Centre of Parallel Forces. Wherever a number of forces of whatever amount act in parallel lines, it is possible to discover a point at which the algebraic sum of the forces may be imagined to act with exactly the same effect. Water in a cistern presses against the side with a pressure varying according to the depth, but always in a direction perpendicular to the side. We may then conceive the whole pressure as exerted on one point, which will be one-third from the bottom of the cistern, and may be called the Centre of Pressure. The Centre of Oscillation of a pendulum, discovered by Huyghens, is that point at which the whole weight of the pendulum may be considered as concentrated, without altering the time of oscillation (see p. 370). Similarly when one body strikes another the Centre of Percussion is that point in the striking body at which all its mass might be concentrated without altering the effect of the stroke. Mathematicians have also described the Centre of Gyration, the Centre of Conversion, the Centre of Friction, &c.

We ought however carefully to distinguish between those circumstances in which an invariable centre can be assigned, and those in which it cannot. In perfect strictness, there is no such thing as a true invariable centre of gravity. As a general rule a body is capable of possessing an invariable centre only for perfectly parallel forces, and gravity never does act in absolutely parallel lines. Thus, as usual, we find that our conceptions are only hypothetically correct, and only approximately applicable to real circumstances. There are indeed certain geometrical forms, called *Centrobaric*[1], such that bodies of that shape would attract each other exactly as if the mass were concentrated at the centre of gravity, whether the forces act in a parallel manner or not. Newton shewed that uniform spheres of matter have this property, and this truth proved of the greatest importance in simplifying his calculations. But it is after all a purely hypothetical truth, because we can nowhere meet with, nor can we construct, a perfectly spherical and homogeneous body. The slightest irregularity or protrusion from the surface will destroy the rigorous correctness of the assumption. The spheroid, on the other hand, has no invariable centre at which its mass may always be regarded as concentrated. The point at which its resultant attraction acts will move about according to the distance and position of the other attracting body, and it will only coincide with the centre as regards an infinitely distant body whose attractive forces may be considered as acting in parallel lines.

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[1] Thomson and Tait, 'Treatise on Natural Philosophy,' vol. i. p. 394.

Physicists speak familiarly of the pole of a magnet, and the term may be used with convenience. But, if we attach any real and definite meaning to it, the pole is not the end of the magnet, nor is it any one fixed point within, but the variable point from which the resultant of all the forces exerted by the particles in the whole bar upon exterior magnetic particles may be considered as acting. The pole is, in short, a Centre of Magnetic Forces; but as those forces are really never parallel, this centre will vary in position according to the relative place of the object attracted. Only when we regard the magnet as attracting a very distant, or, strictly speaking, infinitely distant particle, does the centre become a fixed point, situated in short magnets approximately at one sixth of the whole length from each end of the bar. We have in the above instances of centres or poles of force sufficient examples of the mode in which the Fictitious Mean or Average is employed in physical science.

The Precise Mean Result.

We now turn to that mode of employing the mean result which is analogous to the method of reversal, but which is brought into practice in a most extensive manner throughout many branches of physical science. We find the simplest possible case in the determination of the latitude of a place by observations of the Pole-star. Tycho Brahe suggested that if the elevation of any circumpolar star were observed at its higher and lower passages across the meridian, half the sum of the elevations would be the latitude of the place, which is equal to the height of the pole. Such a star is as much above the pole at its highest point, as it is below at its lowest, so that the mean must necessarily give the height of the pole itself free from doubt, except as regards incidental errors of observation. The Pole-star is usually selected for the purpose of such observations because it describes the smallest circle, and is thus on the whole least affected by atmospheric refraction.

Whenever several causes are in action, each of which at one time increases and at another time decreases the joint effect by equal quantities, we may apply this method and disentangle the effects. Thus the solar and lunar tides roll on in almost complete independence of each other. When the moon is new or full the solar tide coincides, or nearly so, with that caused by the moon, and the joint effect is the sum of the separate effects. When the moon is in quadrature, or half full, the two tides are acting in opposition, one raising and the other depressing the water, so that we observe only the difference of the effects. We have in fact—

$$\text{Spring tide} = \text{lunar tide} + \text{solar tide}$$
$$\text{Neap tide} = \text{lunar tide} - \text{solar tide.}$$

We have only then to add together the heights of the maximum spring tide and the minimum neap tide, and half the sum is the true height of the lunar tide. Half the difference of the spring and neap tides on the other hand gives the solar tide.

Effects of very small amount may with great approach to certainty be detected among much greater fluctuations, provided that we have a series of observations sufficiently numerous and long continued to enable us to balance all the larger effects against each other. For this purpose the observations should be continued over at least one complete cycle, in which the effects run through all their variations, and return exactly to the same relative position as at the commencement. If casual or irregular disturbing causes exist, we should probably require many such cycles of results to render their effect inappreciable. We obtain the desired result by taking the mean of all the observations in which a cause acts positively, and the mean of all in which it acts negatively. Half the difference of these means will be the desired quantity, provided indeed that no other effect happens to vary in the same period.

Since the moon causes so considerable a movement of the ocean, it is evident that its attraction must have some effect upon the atmosphere. The laws of these tides were investigated by Laplace, but as it would be impracticable by theory to calculate their amount, we can only determine them by observation, as Laplace predicted that they would one day be determined[m]. But the oscillations of the barometer thus caused are far smaller than the oscillations due to several other causes. Storms, hurricanes, or changes of weather produce movements of the barometer sometimes as much as a thousand times as great as the tide in question. There are also regular daily, yearly, or other fluctuations, all greater than the desired quantity. To detect and measure the atmospheric tide it was desirable that observations should be made in a place as free as possible from irregular disturbances. On this account several long series of observations were made at St. Helena, where the barometer is far more regular in its movements than in a continental climate. The effect of the moon's attraction was then detected by taking the mean of all the readings when the moon was on the meridian and the similar mean when she was on the horizon. The difference of these means was found to be only ·00365, yet it was possible to discover even the variation of this tide according as the moon was nearer to or further from the earth, though this difference was only ·00056 inch[n]. It is quite evident that such minute effects could never be discovered in a purely empirical manner. Having no information but the series of observations before us, we could have no clue as to the mode of grouping them which would give so small a difference. In applying this method of means in an extensive manner we must generally then have *à priori* knowledge as to the periods at which a cause will act in one direction or the other..

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[m] 'Essai Philosophique sur les Probabilités,' pp. 49, 50.

We are sometimes able to eliminate fluctuations and take a mean result by purely mechanical arrangements. The daily variations of temperature, for instance, become imperceptible one or two feet below the surface of the earth, so that a thermometer placed with its bulb at that depth would give very nearly the true daily mean temperature. At a depth of twenty feet even the yearly fluctuations would become nearly effaced, and the thermometer would stand a little above the true mean temperature of the locality. In registering the rise and fall of the tide by a tide-guage, it is desirable to avoid the oscillations arising from surface waves, which is very readily accomplished by placing the float which marks the level of the water in a cistern communicating by a small hole with the sea. Only a general rise or fall of the level is then perceptible, just as in the marine barometer the narrow tube prevents any casual fluctuations and allows only a continued change of pressure to manifest itself.

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[n] Grant, 'History of Physical Astronomy,' p. 163.

Determination of the Zero point by the Method of Means.

There are a number of important observations in which one of the chief difficulties consists in defining exactly the zero point from which we are to measure. We can point a telescope with great precision to a star and can measure the angle through which the telescope is raised or lowered to a second of arc; but all this precision will be useless unless we can know exactly where the centre point of the heavens is from which we measure, or, what comes to the same thing, the horizontal line 90° distant from it. Since the true horizon has reference to the figure of the earth at the place of observation, we can only determine it by the direction of gravity, as marked either by the plumb-line or the surface of a liquid. The question resolves itself then into the most accurate mode of observing the direction of gravity, and as the plumb-line has long been found hopelessly inaccurate, astronomers generally employ the surface of mercury in repose as the criterion of horizontality. They ingeniously observe the direction of the surface by making a star the index. From the Laws of Reflection it follows that the angle between the direct ray from a star and that reflected from a surface of mercury will be exactly double the angle between the surface and the direct ray from the star. Hence the horizontal or zero point is the mean between the apparent place of any star or other very distant object and its reflection in mercury.

A plumb-line is perpendicular, or a liquid surface is horizontal only in an approximate sense; for any irregularity of the surface of the earth, a mountain, or even a house must cause some deviation by its attracting power. To detect such deviation might seem very difficult, because every other plumb-line or liquid surface would be equally affected by the very principles of gravity. Nevertheless it can be detected; for if we place one plumb-line to the north of a mountain, and another to the south, they will be about equally deflected in opposite directions, and if by observations on the same star we can measure the angle between the plumb-lines, half the inclination will be the deviation of either, after allowance has been made for the inclination due to the difference of latitude of the two places of observation. By this mode of observation applied to the mountain Schehallien the deviation of the plumb-line was accurately measured by Maskelyne, and thus a comparison instituted between the attractive forces of the mountain and the whole globe, which led to a very probable estimate of the earth's average density.

In some cases it is actually better to determine the zero point by the average of equally diverging quantities than by direct observations. Thus in delicate weighings by a chemical balance it is requisite to ascertain exactly the point at which the beam comes to rest, and when standard weights are being compared the position of the beam is ascertained by a carefully divided scale viewed through a microscope. But when the beam is just coming to rest, friction, small impediments or other accidental causes may readily obstruct it, because it is near the point at which the force of stability becomes infinitely small. Hence it is found better to let the beam vibrate and observe the terminal points of the vibrations. The mean between two extreme points will nearly indicate the position of rest. Friction and the resistance of air tend to reduce the vibrations, so that this mean will be erroneous by half the amount of this effect during a half vibration. But by taking several observations we may determine this retardation and allow for it. Thus if a, b, c be the terminal points of three excursions of the beam from the zero of the scale, then $\frac{1}{2}(a+b)$ will be about as much erroneous in one direction as $\frac{1}{2}(b+c)$ in the other, so that the mean of these two means, or what is the same, $\frac{1}{4}(a+2b+c)$, will be exceedingly near to the point of rest[o]. A still closer approximation may be made by taking four readings and reducing them by the formula $\frac{1}{6}(a+2b+2c+d)$.

The accuracy of Baily's experiments, directed to determine the density of the earth, entirely depended upon this mode of observing oscillations. The balls whose gravitation was measured were so delicately suspended by a torsion balance that they never came to rest. The extreme points of the oscillations were observed both when the heavy leaden attracting ball was on one side and on the other. The difference of the mean points when the leaden ball was on the right hand and that when it was on the left hand gave double the amount of the deflection.

A most beautiful instance of the mode of avoiding the use of a zero point is to be found in Mr. E. J. Stone's observations on the radiated heat of the fixed stars. The great difficulty in these observations arose from the comparatively great amounts of heat which were sent into the telescope from the atmosphere, and which were sufficient almost entirely to disguise the feeble heat rays of a star. But Mr. Stone fixed at the focus of his telescope a double thermo-electric pile of which the two parts were reversed in order. Now any disturbance of temperature which acted upon both piles uniformly produced no effect upon the galvanometer needle, and when the rays of the star were made to fall alternately upon one pile and the other, the total amount of the deflection represented double the heating power of the star. Thus Mr. Stone was able to detect with much certainty a heating effect of the star Arcturus, which even when concentrated by the telescope amounted only to $\frac{2}{100}$th of a degree

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[o] Gauss, Taylor's 'Scientific Memoirs,' vol. ii. p. 43, &c.

Fahrenheit, and which represents a heating effect of the direct ray of only about 0°·00000137 Fahrenheit, equivalent to the heat which would be received from a three-inch cubic vessel full of boiling water at the distance of 400 yards[p]. It is probable that Mr. Stone's arrangement of the pile might be usefully employed in other delicate thermometric experiments subject to considerable disturbing influences.

Determination of Maximum Points.

We employ the method of means in a certain number of observations directed to determine the moment at which a phenomenon reaches its highest point in quantity. In noting the place of a fixed star at a given time there is no difficulty in ascertaining the point to be observed, for a star in a good telescope presents an exceedingly small disc. In observing a nebulous body which from a bright centre fades gradually away on all sides, it will not be possible to select with certainty the middle point. In many such cases the best method is not to select arbitrarily the supposed middle point, but points of equal brightness on either side, and then take the mean of the observations of these two points for the centre. As a general rule, a variable quantity in reaching its maximum increases at a less and less rate, and after passing the highest point begins to decrease by insensible degrees. The maximum may indeed be defined as that point at which the increase or decrease is insensibly small. Hence it will usually be the most indefinite point in the whole course, and if we can accurately measure the phenomenon we shall best determine the place of the maximum by determining points on either side at which the ordinates are equal. There is moreover this advantage in the method that several points may be determined with the corresponding ones on the other side, and the mean of the whole taken as the true place of the maximum. But this method entirely depends upon the existence of symmetry in the curve, so that of two equal ordinates one shall be as far on one side of the maximum as the other is on the other side. The method fails when other laws of variation prevail.

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[p] 'Proceedings of the Royal Society,' vol. xviii. p. 159 (Jan. 13, 1870). 'Philosophical Magazine' (4th Series), vol. xxxix. p. 376.

In tidal observations great difficulty is encountered in fixing the moment of high water, because the rate at which the water is then rising or falling is almost imperceptible. Dr. Whewell proposed, therefore, to note the time at which the water passes a fixed point somewhat below the maximum both in rising and falling, and take the mean time as that of high water. But this mode of proceeding unfortunately does not give a correct result, because the tide follows different laws in rising and in falling. There is a difficulty again in selecting the highest spring tide, another object of much importance in tidology. Laplace discovered that the tide of the second day preceding the conjunction of the sun and moon is nearly equal to that of the fifth day following; and, believing that the increase and decrease of the tides proceeded in a nearly symmetrical manner, he decided that the highest tide would occur about thirty-six hours after the conjunction, that is half-way between the second day before and the fifth day after[q].

This method is also employed in determining the time of passage of the middle or densest point of a stream of meteors. The earth takes two or three days in passing completely through the November stream; but astronomers need for their calculations to have some definite point fixed within a few minutes if possible. When near to the middle they observe the numbers of meteors which come within the sphere of vision in each half hour or quarter hour, and then, assuming that the law of variation is symmetrical, they select a moment for the passage of the whole body equidistant between times of equal frequency.

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[q] Airy 'On Tides and Waves,' Encycl. Metrop. pp. 364*–366*.

The eclipses of Jupiter's satellites are not only of great interest as regards the motions of the satellites themselves, but used to be, and perhaps still are, of importance in determining longitudes, because they are events occurring at fixed moments of absolute time, and visible in all parts of the planetary system at the same time, allowance being made for the interval occupied by the light in travelling. But as is excellently explained by Sir John Herschel[r], the moment of the event is wanting in definiteness, partly because the long cone of Jupiter's shadow is surrounded by a penumbra, and partly because the satellite has itself a sensible disc, and takes a certain time in entering the shadow. Different observers using different telescopes would usually select different moments for that of the eclipse. But it is evident that the increase of light in the emersion will proceed according to a law exactly the reverse of that observed in the immersion, so that if an observer notes the time of both events with the same telescope, he will be as much too soon in one observation as he is too late in the other, and the mean moment of the two observations will represent with considerable accuracy the time when the satellite is in the middle of the shadow. The personal error of judgment of the observer is thus eliminated, provided that he takes care to act at the emersion as he did at the immersion.

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[r] 'Outlines of Astronomy,' 4th edition, § 538.

CHAPTER XVII.

THE LAW OF ERROR.

To bring error itself under law might seem beyond human power. He who errs surely diverges from law, and it might well be deemed hopeless to suppose that out of error we can draw truth. One of the most remarkable achievements of the human intellect is the establishment of a general theory which not only enables us among discrepant results to approximate to the truth, but to assign the degree of probability which fairly attaches to this conclusion. It would be a gross misapprehension indeed to suppose that this law is necessarily the best guide under all circumstances. Every measuring instrument and every form of experiment may have its own special law of error; there may in one instrument be a tendency in one direction and in another in the opposite direction. Every process has its peculiar liabilities to mistake and disturbance, and we are never relieved from the necessity of vigilantly providing against such special difficulties. The general Law of Error is the best guide only when we have exhausted all other means of approximation, and still find discrepancies, which are due to entirely unknown causes. We must treat such residual differences in some way or other, since they will occur in all accurate experiments, and as their peculiar nature and origin is assumed to be unknown, there is no reason why we should treat them differently in different cases. Accordingly the ultimate Law of Error must be a uniform and general one.

It is perfectly recognised by mathematicians that in each special case a special Law of Error may apply, and should be discovered and adopted if possible. 'Nothing can be more unlikely than that the errors committed in all classes of observations should follow the same law[a],' and the special Laws of Error which will apply to certain instruments, as for instance the repeating circle, have been investigated by M. Bravais[b]. He concludes that every partial and distinct cause of error gives rise to a curve of possibility of errors, which may have any form whatever,—a curve which we may either be able or unable to discover, and which in the first case may be determined by considerations *à priori*, on the peculiar nature of this cause, or which may be determined *à posteriori* by observation. Whenever it is practicable and worth the labour, we ought to investigate these special conditions of error; nevertheless, when there are a great number of different sources of minute error, the general resultant will always tend to obey that general law which we are about to consider.

Establishment of the Law of Error.

Mathematicians agree far better as to the nature of the ultimate Law of Error than they do as to the manner in which it can be deduced and proved. They agree that among a number of discrepant results of observation, that mean quantity is probably the most nearly approximate to the truth which makes the sum of the squares of the errors as small as possible. But there are at least three different ways in which this principle has been arrived at respectively by Gauss, by Laplace, by Quetelet and by Sir John Herschel. Gauss proceeds much upon assumption; Herschel rests upon geometrical considerations; while Laplace and Quetelet regard the Law of Error as a development of the doctrine of combinations; that of Gauss may be first noticed.

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[a] 'Philosophical Magazine,' 3rd Series, vol. xxxvii. p. 324.

[b] 'Letters on the Theory of Probabilities,' by Quetelet, transl. by O. G. Downes, Notes to Letter XXVI. pp. 286–295.

The Law of Error expresses the comparative probability of errors of various magnitude, and partly from experience, partly from *à priori* considerations, we may readily lay down certain conditions to which the law will certainly conform. It may fairly be assumed as a first principle to guide us in the selection of the law, that large errors will be far less frequent and probable than small ones. We know that very large errors are almost impossible, so that the probability must rapidly decrease as the amount of the error increases. A second principle is that positive and negative errors shall be equally probable, which may certainly be assumed, because we are supposed to be devoid of any knowledge as to the causes of the residual errors. It follows that the probability of the error must be a function of an even power of the magnitude, that is of the square, or the fourth power, or the sixth power, otherwise the probability of the same amount of error would vary accordingly as the error was positive or negative. The even powers x^2, x^4, x^6, &c., are always intrinsically positive, whether x be positive or negative. There is no *à priori* reason why one rather than another of these even powers should be selected. Gauss himself allows that the fourth or sixth powers would fulfil the conditions as well as the second[c], but in the absence of any theoretical reasons we should prefer the second power, because it leads to formulæ of great comparative simplicity. Did the Law of Error necessitate the use of the higher powers of the error, the complexity of the necessary calculations would much reduce the utility of the theory.

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[c] 'Méthode des Moindres Carrés.' 'Mémoires sur la Combinaison des Observations, par Ch. Fr. Gauss. Traduit en Français par J. Bertrand,' Paris, 1855, pp. 6, 133, &c.

By a process of reasoning, which it would be undesirable to attempt to follow in detail in this place, it is shown that, under these conditions, the most probable result of any series of recorded observations is that which makes the sum of the squares of the errors the least possible. Let a, b, c, &c., be the results of observation, and x the quantity selected as the most probable, that is the most free from unknown errors: then we must determine x so that $(a-x)^2+(b-x)^2+(c-x)^2+\dots$ shall be the least possible quantity. Thus we arrive at the celebrated *Method of Least Squares*, as it is usually called, which appears to have been first distinctly put in practice by Gauss in 1795, while Legendre first published in 1806 an account of the process in his work, entitled, 'Nouvelles Méthodes pour la détermination des Orbites des Comètes.' It is worthy of notice, however, that Roger Cotes had long previously recommended a method of equivalent nature in his tract, 'Estimatio Erroris in Mixta Mathesi[d].'

Herschel's Geometrical Proof.

A second method of demonstrating the Principle of Least Squares was proposed by Sir John Herschel, and although only applicable to geometrical notions, it is remarkable as showing that from whatever point of view we regard the subject, the same principle will be detected. After assuming that some general law must exist, and that it is subject to the general principles of probability, he supposes that a ball is dropped from a high point with the intention that it shall strike a given mark on a horizontal plane. In the absence of any known causes of deviation it will either strike that mark, or, as

[d] De Morgan, 'Penny Cyclopædia,' art. *Least Squares*.

is infinitely more probable, diverge from it by an amount which we must regard as error of unknown origin. Now, to quote the words of Sir J. Herschel[e], 'the probability of that error is the unknown function of its square, i. e. of the sum of the squares of its deviations in any two rectangular directions. Now, the probability of any deviation depending solely on its magnitude, and not on its direction, it follows that the probability of each of these rectangular deviations must be the same function of *its* square. And since the observed oblique deviation is equivalent to the two rectangular ones, supposed concurrent, and which are essentially independent of one another, and is, therefore, a compound event of which they are the simple independent constituents, therefore its probability will be the product of their separate probabilities. Thus the form of our unknown function comes to be determined from this condition, viz., that the product of such functions of two independent elements is equal to the same function of their sum. But it is shown in every work on algebra that this property is the peculiar characteristic of, and belongs only to, the exponential or antilogarithmic function. This, then, is the function of the square of the error, which expresses the probability of committing that error. That probability decreases, therefore, in geometrical progression, as the square of the error increases in arithmetical.'

Laplace's and Quetelet's Proof of the Law of Error.

However much presumption the modes of determining the Law of Error, already described, may give in favour of the law usually adopted, it is difficult to feel that the arguments are satisfactory and conclusive. The law adopted is chosen rather on the grounds of convenience and plausibility, than because it can be seen to be the true and necessary law. We can however approach the subject from an entirely different point of view, and yet get to the same result.

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[e] 'Edinburgh Review,' July 1850, vol. xcii. p. 17. Reprinted 'Essays,' p. 399. This method of demonstration is discussed by Boole, 'Transactions of Royal Society of Edinburgh,' vol. xxi. pp. 627–630.

Let us assume that a particular observation is subject to four chances of error, each of which will increase the result one inch if it occurs. Each of these errors is to be regarded as an event independent of the rest and we can therefore assign, by the theory of probability, the comparative probability and frequency of each conjunction of errors. From the Arithmetical Triangle (pp. 208, 213) we learn that the ways of happening are as follows:—

No error at all	1 way.
Error of 1 inch	4 ways.
Error of 2 inches	6 ways.
Error of 3 inches	4 ways.
Error of 4 inches	1 way.

We may infer that the error of two inches is the most likely to occur, and will occur in the long run in six cases out of sixteen. Errors of one and three inches will be equally likely, but will occur less frequently; while no error at all, or one of four inches will be a comparatively rare occurrence. If we now suppose the errors to act as often in one direction as the other, the effect will be to alter the average error by the amount of two inches, and we shall have the following results:—

Negative error of 2 inches	1 way.
Negative error of 1 inch	4 ways.
No error at all	6 ways.
Positive error of 1 inch	4 ways.
Positive error of 2 inches	1 way.

We may now imagine the number of causes of error increased and the amount of each error decreased, and the arithmetical triangle will always give us the proportional frequency of the resulting errors. Thus if there be five positive causes of error and five negative causes, the following table shows the comparative numbers of aggregate errors of various amount which will be the result:—

Direction of Error.	Positive Error.		Negative Error.
Amount of Error.	5, 4, 3, 2, 1	0	1, 2, 3, 4, 5
Number of such Errors.	1, 10, 45, 120, 210	252	210, 120, 45, 10, 1

It is plain that from such numbers I can ascertain the probability of any particular amount of error under the conditions supposed. Thus the probability of a positive error of exactly one inch is $\frac{210}{1024}$, in which fraction the numerator is the exact number of combinations giving one inch positive error, and the denominator the whole number of possible errors of all magnitudes. I can also, by adding together the appropriate numbers, get the probability of an error not exceeding a certain amount. Thus the probability of an error of three inches or less, positive or negative, is a fraction whose numerator is the sum of $45+120+210+252+210+120+45$, and the denominator, as before, giving the result $\frac{1002}{1024}$.

We may see at once that, according to these principles, the probability of small errors is far greater than of large ones: thus the odds are 1002 to 22, or more than 45 to 1, that the error will not exceed three inches; and the odds are 1022 to 2 against the occurrence of the greatest possible error of five inches. The existence of no error at all is the most likely event; but a small error, such as that of one inch positive, is little less likely.

If any case should arise in which the observer knows the number and magnitude of the independent errors which may occur, he ought certainly to calculate from the Arithmetical Triangle the special Law of Error which would apply. But the general law, of which we are in search, is to be used in the dark, when we have no knowledge whatever of the sources of error. To assume any special number of causes of error is then an arbitrary proceeding, and mathematicians have chosen the least arbitrary course of imagining the existence of an infinite number of infinitely small errors, just as, in the inverse method of probabilities, an infinite number of infinitely improbable hypotheses were submitted to calculation (p. 296).

The reasons in favour of this choice are of several different kinds.

1. It cannot be denied that there may exist infinitely numerous causes of error in any act of observation.

2. The resulting law on the hypothesis of a large finite, or even a moderate finite number of causes of error, does not appreciably differ from that given by the hypothesis of infinity.

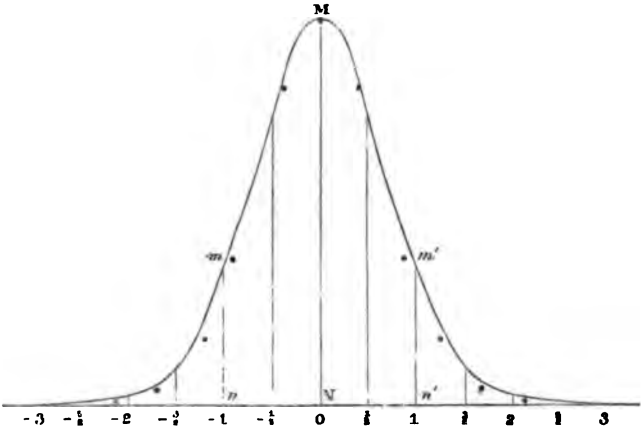
3. We gain by the hypothesis of infinity a general law capable of ready calculation, and applicable by uniform rules to all problems.

4. This law, when tested by comparison with extensive series of observations, is strikingly verified, as will be shown in a later section.

When we imagine the existence of any large number of causes of error, for instance one hundred, the numbers of combinations become impracticably large, as may be seen to be the case from a glance at the Arithmetical Triangle (p. 208), which proceeds only up to the seventeenth line. M. Quetelet, by suitable abbreviating processes, succeeded in calculating out a table of probability of errors on the hypothesis of one thousand distinct causes[f]; but mathematicians have generally proceeded on the hypothesis of infinity, and then, by some of the beautiful devices of analysis, have substituted a general law of easy treatment. In mathematical works upon the subject, it is shown that the standard Law of Error is expressed in the formula

$$y = Y\epsilon^{-cx^2},$$

in which x is the amount of the error, Y the maximum ordinate of the curve of error, and c a number constant for each series of observations, and expressing the general amount of the tendency to error, but varying between one series of observations and another, while ϵ is the peculiar constant, 2·71828 ..... the base of the Naperian logarithms. To show the close correspondence of this general law with the special law which might be derived from the supposition of any moderate number of causes of error, I have in the accompanying figure drawn a curved line representing accurately the variation of y when x in the above formula is taken equal to 0, $\frac{1}{2}$, 1, $\frac{3}{2}$, 2, &c., positive or negative, the arbitrary quantities Y and c



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[f] 'Letters on the Theory of Probabilities,' Letter XV. and Appendix, note pp. 256–266.

being both assumed equal to unity, in order to simplify the calculations. In the same figure are inserted eleven dots, whose heights above the base line are proportional to the numbers in the eleventh line of the Arithmetical Triangle, thus representing the comparative probabilities of errors of various amounts arising from ten equal causes of error. It is apparent that the correspondence of the general and the special Law of Error is almost as close as can be exhibited in the figure, and the assumption of a greater number of equal causes of error would render the correspondence far more close.

It may be explained that the ordinates, for instance NM, *nm*, *n'm'*, represent values of *y* in the equation expressing the Law of Error. The occurrence of any one definite amount of error is infinitely improbable, because an infinite number of such ordinates might be drawn. But the probability of an error occurring between certain definite limits is finite, and is represented by a portion of the *area* of the curve. Thus the probability that an error, positive or negative, not exceeding unity will occur, is represented by the area M*mnn'm'*, in short, by the area standing upon the line *nn'*. Since every observation must either have some definite error or none at all, it follows that the whole area of the curve should be considered as the unit expressing certainty, and the probability of an error falling between particular limits will then be expressed by the ratio which the area of the curve between those limits bears to the whole area of the curve.

Derivation of the Law of Error from Simple Logical Principles.

It is worthy of notice that this Law of Error, abstruse though the subject may seem, is really founded upon the simplest principles. It arises entirely out of the difference between permutations and combinations, a subject upon which I may seem to have dwelt with unnecessary prolixity in previous pages (pp. 200–216). The order in which we add quantities together does not affect the amount of the sum, so that if there be three positive and five negative causes of error in operation, it does not matter in which order they are considered as acting. They may be indifferently intermixed in any arrangement, and yet the result will be the same. The reader should not fail to notice how laws or principles which appeared to be absurdly simple and evident when first noticed, reappear in the most complicated and mysterious processes of scientific method. The fundamental Laws of Identity and Difference gave rise to the Logical Abecedarium, which, after abstracting the character of the differences, led to the Arithmetical Triangle (p. 214). The Law of Error is defined by an infinitely high line of that triangle, and the law proves that the mean is the most probable result, and that divergencies from the mean become much less probable as they increase in amount. Now the comparative greatness of the numbers towards the middle of each line of the Arithmetical Triangle is entirely due to the indifference of order in space or time, which was first prominently pointed out as a condition of logical relations, and the symbols indicating them (pp. 40–42), and which was afterwards shown to attach equally to numerical symbols, the derivatives of logical terms (pp. 180, 181).

Verification of the Law of Error.

The theory of error which we have been considering rests entirely upon an assumption, namely that when known sources of disturbances are allowed for, there yet remain an indefinite, possibly an infinite number of other minute sources of error, which will as often produce excess as deficiency. Granting this assumption, the Law of Error must be as it is usually taken to be, and there is no more need to verify empirically than to test the truth of one of Euclid's propositions mechanically, after we have proved it theoretically. Nevertheless, it is an interesting occupation to verify even the propositions of geometry in an approximate manner, and it is still more instructive to inquire whether a large number of observations will be found to justify our assumption of the Law of Error.

Encke has given an excellent instance of the correspondence of theory with experience, in the case of certain observations of the difference of Right Ascension of the sun and two stars, namely α Aquilæ and α Canis minoris. The observations were 470 in number, and were made by Bradley and reduced by Bessel, who found the probable error of the final result to be only about one-fourth part of a second (0″·2637). He then compared the number of errors of each magnitude from $\frac{1}{10}$th part of a second upwards, as actually given by the observations, with what should occur according to the Law of Error.

The results were as follow[g]:—

Magnitude of the errors in parts of a second.	Number of errors of each magnitude according to	
	Observation.	Theory.
0·0 to 0·1	94	95
·1 „ ·2	88	89
·2 „ ·3	78	78
·3 „ ·4	58	64
·4 „ ·5	51	50
·5 „ ·6	36	36
·6 „ ·7	26	24
·7 „ ·8	14	15
·8 „ ·9	10	9
·9 „ 1·0	7	5
above „ 1·0	8	5

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[g] Encke, 'On the Method of Least Squares,' Taylor's 'Scientific Memoirs,' vol. ii. pp. 338, 339.

The reader will remark that the correspondence is remarkably close, except as regards larger errors, which are excessive in practice. It is one objection, indeed, to the theory of error, that, being expressed in a continuous mathematical function, it contemplates the possible existence of errors of every magnitude, such indeed as could not practically occur ; yet in this case the theory seems to under-estimate the number of large errors.

Another excellent comparison of the law with observation has been made by Quetelet, who has investigated the errors of 487 determinations in time of the Right Ascension of the Pole-star, made at Greenwich during the four years 1836–39. These observations, although carefully corrected for all known causes of error, as well as for nutation, precession, &c., are yet of course found to differ, and being classified as regards intervals of one-half second of time, and then proportionately increased in number, so that their sum may be one thousand, give the following results as compared with what theory would lead us to expect[h] :—

Magnitude of error in tenths of a second.	Number of errors		Magnitude of error in tenths of a second.	Number of errors	
	by Observation.	by Theory.		by Observation.	by Theory.
0·0	168	163	—	—	—
+ 0·5	148	147	− 0·5	150	152
+ 1·0	129	112	− 1·0	126	121
+ 1·5	78	72	− 1·5	74	82
+ 2·0	33	40	− 2·0	43	46
+ 2·5	10	19	− 2·5	25	22
+ 3·0	2	10	− 3·0	12	10
—	—	—	− 3·5	2	4

In this instance the correspondence is also satisfactory, but the divergence between theory and fact is in the opposite direction to that discovered in the former comparison, the larger errors being less frequent than theory would indicate.

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[h] Quetelet, 'Letters on the Theory of Probabilities,' translated by Downes, Letter XIX. p. 88. See also Galton's 'Hereditary Genius,' p. 379.

We may also regard the experiments enumerated in the chapter on Probabilities (p. 238), as forming an empirical verification of the theory of error.

Remarks on the General Law of Error.

The mere fact that the Law of Error allows of the possible existence of errors of every assignable amount shows that it is only approximately true. We may fairly say that in measuring a mile it would be impossible to commit an error of a hundred miles, and the length of life would never allow of our committing an error of one million ~~miles~~ years. Nevertheless the general Law of Error would assign a probability for an error of that amount or more, but so small a probability as to be utterly inconsiderable, and almost inconceivable. All that can, or in fact need, be said in defence of the law is, that it may be made to represent the errors in any special case to a very close approximation, and that the probability of large and practically impossible errors, as given by the law, will be so small as to be entirely inconsiderable. And as we are dealing with error itself, and our results pretend to nothing more than approximation and probability, an indefinitely small error in our process of approximation is of no importance whatever.

The Probable Mean Result as defined by the Law of Error.

One immediate result of the Law of Error, as thus stated, is that the mean result is the most probable one ; and when there is only a single variable this mean is found by the familiar arithmetical process. An unfortunate error has crept into several works which allude to this subject. Mr. Mill, in treating of the 'Elimination of Chance,' remarks in a note[i] that 'the mean is spoken of as if it were exactly the same thing with the average. But the mean, for purposes of inductive inquiry, is not the average, or arithmetical mean, though in a familiar illustration of the theory the difference may be disregarded.' He goes on to say that, according to mathematical principles, the most probable result is that for which the sums of the squares of the deviations is the least possible. In Bowen's 'Treatise on Logic' (p. 439), we find the Method of Least Squares mentioned as 'a mode of finding the most probable result in those cases in which the arithmetical mean is not an applicable expedient for determining the probability.' It seems probable that these and other writers were misled by Dr. Whewell's remarks on the subject; for he says[k] that 'The Method of Least Squares is in fact a Method of Means, but with some peculiar characters. . . . The method proceeds upon this supposition; that all errors are not equally probable, but that small errors are more probable than large ones.' He adds that this method 'removes much that is arbitrary in the Method of Means.' It is strange to find a mathematician like Dr. Whewell making such remarks, when there is no doubt whatever that the Method of Means is only an application of the Method of Least Squares. They are, in fact, the same method, except that the latter method may be applied to cases where two or more quantities have to be determined at the same time. Many authorities might be quoted to this effect, but it will be sufficient to mention Lubbock and Drinkwater, who say[l], 'If only one quantity has to be determined, this method evidently resolves itself into taking the mean of all the values given by observation.' Encke, again, distinctly says[m], that the expression for the probability of an error 'not only contains in itself the principle of the arithmetical mean, but depends so immediately upon it, that for all those magnitudes for which the arithmetical mean holds good in the simple cases in which it is principally applied, no other law of probability can be assumed than that which is expressed by this formula.'

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[i] 'System of Logic,' bk. iii. chap. 17, § 3. 5th ed. vol. ii. p. 56.

[k] 'Philosophy of the Inductive Sciences,' 2nd ed. vol. ii. pp. 408, 409.

[l] 'Essay on Probability,' by J. W. Lubbock and J. E. Drinkwater, Useful Knowledge Society, 1833, p. 41.

It can be shown, too, in a moment that the mean is the result which gives the least sum of squares of errors. For if a, b, c, &c., be the results of observation and x the selected mean result, the sum of squares of the errors is $(a-x)^2+(b-x)^2+(c-x)^2+$ &c., which is at a minimum when its differential coefficient $2(a-x+b-x+c-x+$ &c.$)=0$. From this equation we immediately obtain, denoting by n the number of separate results, a, b, c, &c., $x=(a+b+c+\dots)\frac{1}{n}$, or the ordinary arithmetic mean.

Weighted Observations.

It is to be distinctly understood that when we take the mean of certain numerical results as the most probable number aimed at, we regard all the different results as equally good and probable in themselves. The theory of error expresses no preference for any one number over any other. If, then, an observer has reason to suppose that some results are not so trustworthy as others, he must take account of this difference in drawing the mean. By the method of *weighting* observations this difference of value is easily allowed for. Astronomers are in the habit of recording with an observation the value or degree of confidence with which they regard it, freely estimated according to the impression of success or failure in accuracy *immediately after the observation.* This value is usually expressed in a decimal scale, so that 10 denotes the highest degree of satisfaction with the result, and 1 the least degree. Before taking the mean of the observations each number is multiplied by its weight or value, and the sum of the products is divided by the sum of the weights. Thus if a, b, c, &c., be the observed numbers, and w, w', w'', &c., the weights, then the most probable mean is $\frac{aw + bw' + cw'' + \dots}{w + w' + w'' + \dots}$. This formula, it will be observed, is identical in form with that for finding the centre of gravity of particles of different weights arranged in a straight line. When we regard w, w', w'', &c., as all equal, it becomes identical with the formula for the ordinary mean. This method of weighting observations, now of much importance in astronomical and other very exactly quantitative investigations, appears to have been first proposed by Roger Cotes, the editor of the 'Principia,' as pointed out by De Morgan[n].

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[m] Taylor's 'Scientific Memoirs,' vol. ii. p. 333.

The practice of giving weights would open the way to much error and abuse, if the weights were assigned when the mean was being drawn, and when the divergence of some results from the others would be likely to become the guide. As a general rule the weights must be assigned at the moment of observation, and afterwards rigidly maintained, and they must be assigned not from regard to the apparent intrinsic accuracy of the result, but the extrinsic circumstances which seem to render it valuable. An observed result, in short, must be discredited, not because it is divergent, but because there were other reasons to suppose that it would be divergent.

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[n] 'Penny Cyclopædia,' art. *Least Squares.*

The Probable Error of Mean Results.

When we draw any conclusion from the numerical results of observations we ought not to consider it sufficient, in cases of importance, to content ourselves with finding the simple mean and treating it as true. We ought also to ascertain what is the degree of confidence we may place in this mean, and our confidence should be measured by the degree of concurrence of the observations from which it is derived. In some cases the mean may be so close to the correct result that we may consider it as approximately certain and accurate. In other cases it may really be worth little or nothing. The Law of Error enables us to give exact expression to the degree of confidence proper in any case; for it shows how to calculate the probability of a divergence of any amount from the mean, and we can thence ascertain the probability that the mean in question is within a certain distance from the true number. The *probable error* is taken by mathematicians to mean the limits within which it is as likely as not that the truth will fall. Thus if 5·45 be the mean of all the determinations of the density of the earth, and ·20 be approximately the probable error, the meaning is that the probability of the real density of the earth falling between 5·25 and 5·65 is ½. Any other limits might have been selected at will. We might readily calculate the limits within which it was one hundred or one thousand to one that the truth would fall; but there is a general convention to take the even odds, one to one, as the quantity of probability of which the limits are to be estimated.

Many books on the subject of probability give rules for making the calculations, but as, in the gradual progress of science, all persons ought to be more familiar with these processes, I propose to repeat the rules here and illustrate their use. The calculations, when made in strict accordance with the directions, involve none but arithmetic operatio

Rules for finding the probable error of a mean result:—

1. Draw the mean of all the observed results.

2. Find the excess or defect, that is, the error of each result from the mean.

3. Square each of these reputed errors.

4. Add together all these squares of the errors.

5. Take the square root of this sum.

6. Divide the square root by the number of results.

7. Multiply the quotient by 0·67449 (or approximately by 0·674, or even 0·67), a natural constant number derived from the Law of Error in a manner which is described in mathematical works upon the subject.

Suppose, for instance, that five measurements of the height of a hill, by the barometer or otherwise, have given the numbers of feet as 293, 301, 306, 307, 313; we want to know the probable error of the mean, namely 304. Now the differences between this mean and the above numbers, *paying no regard to direction*, are 11, 3, 2, 3, 9; their squares are 121, 9, 4, 9, 81, and the sum of the squares consequently 224. Taking the square root of this sum by the common arithmetic process, or by logarithms, we obtain 14·966, and dividing by five, the number of observations, we have 2·99, which has only to be multiplied by ·67 to yield us 2·019. This number is so close to 2, that we may call the probable error equal to two. Thus the probability is one-half, or the odds are even, that the true height of the mountain lies between 302 and 306 feet. We have thus an exact measure of the degree of credibility of our mean result, which mean indicates the most likely point for the truth to fall upon.

The reader should observe that as the object in these calculations is only to gain a notion of the degree of confidence with which we view the mean, there is no real use in carrying the calculations to any great degree of precision; and whenever the neglecting of decimal fractions, or even the slight alteration of a number will much abbreviate the computations, it may be fearlessly done, except in cases of high importance and precision. It has been stated that the voyages of the Great Britain steamship to Melbourne from Liverpool, up to May, 1871, have been thirteen in number, with the following durations in days: 62, 63, 59, 60, 58, 61, 57, 57, 57, 57, 56, 63, 55. The mean duration of the voyages is 58·85 days, which is the most probable length of any similar future voyage; but to calculate the probable error, we may take the mean to be 59 days. The sum of the squares of the errors is only 88, and the probable error thence calculated 0·49 day, or, say half a day. It is as likely as not, then, that any particular voyage will be not less than 58½ days, nor more than 59½ days.

The experiments of Benzenberg to detect the revolution of the earth, by the deviation of a ball from the exact perpendicular line in falling down a deep pit, have been cited by Encke[o] as an interesting illustration of the Law of Error. The mean deviation was 5·086 lines, and its probable error was calculated by Encke to be not more than ·950 line, that is, the odds were even that the true result lay between 4·136 and 6·036. As the deviation should, according to astronomical theory be 4·6 lines, which lies well within the limits, we may consider that the experiments are consistent with the Copernican system of the universe.

It will of course be understood that the probable error has regard only to the differences of the results from which the mean is drawn, and takes no account of constant errors. The true result accordingly will often fall far beyond the limits of probable error.

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[o] Taylor's 'Scientific Memoirs,' vol. ii. pp. 330, 347, &c.

The Rejection of the Mean Result.

We ought always to bear in mind that the mean of any series of observations is the best, that is, the most probable approximation to the truth, only in the entire absence of any knowledge to the contrary. The selection of the mean rests entirely upon the probability that wholly unknown causes of error will in the long run fall as often in one direction as the opposite, so that in drawing the mean they will balance each other. If we have any presumption to the contrary, any reason to suppose that there exists a tendency to error in one direction rather than the other, then to choose the mean would be to ignore that tendency. Thus we may certainly approximate to the length of the circumference of a circle, by measuring the perimeters of inscribed and circumscribed polygons of an equal and large number of sides. The correct length of the circular line undoubtedly lies between the lengths of the two perimeters, but it does not follow that the mean is the best approximation. It may in fact be shown upon mathematical principles that the circumference of the circle is *very nearly* equal to the perimeter of the inscribed polygon, together with one-third part of the difference between the inscribed and circumscribed polygons of the same number of sides. Having this knowledge we ought of course to act upon it, instead of upon vague grounds of probability.

We may often perceive that a series of measurements tends towards an extreme limit rather than towards a mean. Thus in endeavouring to obtain a correct estimate of the apparent diameter of the brightest fixed stars, we should find a continuous diminution in estimates as the powers of observation increased. Kepler assigned to Sirius an apparent diameter of 240 seconds; Tycho Brahe made it 126; Gassendi 10 seconds; Galileo, Hevelius, and J. Cassini, 5 or 6 seconds. Halley, Michell, and subsequently Sir W. Herschel came to the conclusion that the brightest stars in the heavens could not have real discs of a second, and were probably much less in diameter. It would of course be absurd to take the mean of quantities which differ more than 240 times; and as the tendency has always been to smaller estimates, there is a considerable indication in favour of the smallest[p].

In the case of many experiments and measurements we shall know on which side there is a tendency to error. Thus the readings of a thermometer always tend to rise as the age of the instrument increases, and no drawing of means will correct this result. Barometers, on the other hand, are always likely to read too low instead of too high, owing to the imperfection of the vacuum, or the action of capillary attraction. If the mercury be perfectly pure and no considerable error be due to the measuring apparatus, the best barometer will be that which gives the highest result.

When we have reasonable grounds for supposing that certain experimental results are liable to grave errors, we should exclude them in drawing a mean. If we want to find the most probable approximation to the velocity of sound in air, it would be absurd to go back to the old experiments which made the velocity from 1200 to 1474 feet per second; for we know that the old observers did not guard against errors arising from wind and other causes. Old chemical experiments are absolutely valueless as regards quantitative results. The old chemists found the atmosphere to differ in composition nearly ten per cent. in different places, whereas modern accurate experimenters find very slight variations. Any method of measurement which we know to avoid a source of error is far to be preferred to others which trust to probabilities for the elimination of the error. As Flamsteed says[q], 'One good instrument is of as much worth as a hundred indifferent ones.' But an instrument is good or bad only in a comparative sense, and no instrument gives invariable and truthful results. Hence we must always ultimately fall back upon general probabilities for the selection of the final mean, when our other precautions are exhausted.

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[p] Quetelet, 'Letters,' &c. p 116.

Very difficult questions sometimes arise when one or more results of a method of experiment diverge widely from the mean of the rest. Are we or are we not to exclude them in adopting the supposed true mean result of the method. The drawing of a mean result rests, as I have frequently explained, upon the assumption that every error acting in one direction will probably be balanced by other errors acting in an opposite direction. If then we know or can possibly discover any causes of error not agreeing with this assumption, we shall be justified in excluding results which seem to be affected by this cause.

In reducing large series of astronomical observations, it is not uncommon to meet with numbers differing from others by a whole degree or half a degree, or some considerable integral quantity. These are errors which could hardly arise in the act of observation or in instrumental irregularity; but they might readily be accounted for by misreading of figures or mistaking of division marks. It would be absurd to trust to chance that such mistakes would balance each other in the long run, and it is therefore better to correct arbitrarily the supposed mistake, or better still, if new observations can be made, to strike out the divergent numbers altogether. When results come sometimes too great or too small in a regular manner, we should suspect that some part of the instrument slips through a definite space, or that a definite cause of error enters at times, and not at others. We should then make it a point of prime importance to discover the exact nature and amount of such an error, and either prevent its occurrence for the future or else introduce a corresponding correction. In many researches the whole difficulty will consist in this detection and avoidance of sources of error. Thus Professor Roscoe found that the presence of phosphorus caused serious and almost unavoidable errors in the determination of the atomic weight of vanadium[r]. Sir John Herschel, in reducing his observations of double stars at the Cape of Good Hope, was perplexed by an unaccountable difference of the angles of position as measured by the Seven-feet Equatorial and the Twenty-feet Reflector Telescopes, and after a careful investigation was obliged to be contented with introducing a correction experimentally determined[s].

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[q] Baily, 'Account of Flamsteed,' p. 56.

Even the most patient and exhaustive investigations will sometimes fail to disclose any reason why some results diverge in an unusual and unexpected manner from others. The question again recurs—Are we arbitrarily to exclude them? The answer should be in the negative as a general rule. The mere fact of divergence ought not to be taken as conclusive against a result, and the exertion of arbitrary choice would open the way to the most fatal influence of bias, and what is commonly known as the 'cooking' of figures. It would amount in most cases to judging fact by theory instead of theory by fact. The apparently divergent number may even prove in time to be the true one. It may be an exception of that peculiarly valuable kind which upsets our false theories, a real exception, exploding apparent coincidences, and opening the way to a wholly new view of the subject. To establish this position for the divergent fact will of course require additional research; but in the meantime we should give it a fair weight in our mean conclusions, and should bear in mind the discrepancy as one demanding attention. To neglect a divergent result is to neglect the possible clue to a great discovery.

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[r] Bakerian Lecture, 'Philosophical Transactions' (1868), vol. clviii. p. 6.

[s] 'Results of Observations at the Cape of Good Hope,' p. 283.

Method of Least Squares.

When two or more unknown quantities are so involved that they cannot be separately determined by the single Method of Means, we can yet obtain their most probable amounts by the Method of Least Squares, without more difficulty than arises from the length of the arithmetical computations. If the result of each observation gives an equation between two unknown quantities of the form

$$ax + by = c$$

then, if the observations were free from error, we should only need two observations giving two equations; but, for the attainment of greater accuracy, we may take a series of observations, and then reduce the equations so as to give only a pair with average coefficients. This reduction is effected by, firstly, multiplying the coefficients of each equation by the first coefficient, and adding together all the similar coefficients thus resulting for the coefficients of a new equation; and secondly, by repeating this process, and multiplying the coefficients of each equation by the coefficient of the second term. Thus meaning by (sum of a^2) the sum of all quantities of the same kind, and having the same place in the equations as a^2, we may briefly describe the two resulting mean equations as follows:—

$$(\text{sum of } a^2) \,.\, x + (\text{sum of } ab) \,.\, y = (\text{sum of } ac),$$
$$(\text{sum of } ab) \,.\, x + (\text{sum of } b^2) \,.\, y = (\text{sum of } bc).$$

When there are three or more unknown quantities the process is exactly the same in nature, and we only need additional mean equations to be obtained by multiplying by the third, fourth, &c., coefficients. As the numbers are in any case only approximate, it is usually quite unnecessary to make the computations with any great degree of accuracy, and places of decimals may therefore be freely cut off to save arithmetical work. The mean equations having been computed, their solution by the ordinary methods of algebra gives the most probable values of the unknown quantities.

Works upon the Theory of Probability and the Law of Error.

Regarding the Theory of Probability and the Law of Error as constituting, perhaps, the most important subjects of study for any one who desires to obtain a complete comprehension of logical and scientific method as actually applied in physical investigations, I will briefly indicate the works in one or other of which the reader will best pursue the study.

The best popular, and at the same time profound English work on the subject is De Morgan's 'Essay on Probabilities and on their Application to Life Contingencies and Insurance Offices,' published in the 'Cabinet Cyclopædia,' and to be obtained from Messrs. Longman. No mathematical knowledge beyond that of common arithmetic is required in reading this work. Quetelet's 'Letters,' already often referred to, also form a most interesting and excellent popular introduction to the subject, and the mathematical notes are also of value. Sir George Airy's brief treatise 'On the Algebraical and Numerical Theory of Errors of Observation and the Combination of Observations,' contains a complete explanation of the Law of Error and its practical applications. De Morgan's treatise 'On the Theory of Probabilities' in the 'Encyclopædia Metropolitana,' presents an abstract of the more abstruse investigations of Laplace, together with a multitude of profound and original remarks concerning the theory generally. In Lubbock and Drinkwater's work on 'Probability,' in the Library of Useful Knowledge, we have a very concise but good statement of a number of important problems. The Rev. W. A. Whitworth has given, in an interesting little work entitled 'Choice and Chance,' a number of good illustrations of the calculations both in the theories of Combinations and Probabilities. In Mr. Todhunter's admirable History we have an exhaustive critical account of almost all writings upon the subject of probability down to the culmination of the theory in Laplace's works. In spite of the existence of these and some other good English works, there seems to be a want of an easy and yet pretty complete introduction to the study of the theory of probabilities.

Among French works the 'Traité Élémentaire du Calcul des Probabilités,' by S. F. Lacroix, of which several editions have been published, and which is not difficult to obtain, forms probably the best elementary treatise. Poisson's 'Recherches sur la Probabilité des Jugements,' (Paris, 1837), commences with an admirable investigation of the grounds and methods of the theory. While Laplace's great 'Théorie Analytique des Probabilités' is of course the 'Principia' of the subject, his 'Essai Philosophique sur les Probabilités' is a popular discourse, and is one of the most profound and interesting essays ever published. It should be familiar to every student of logical method, and has lost little or none of its importance by lapse of time.

Detection of Constant Errors.

The Method of Means is absolutely incapable of eliminating any error which is always the same, and which always lies in one direction. We sometimes require to be aroused from a false feeling of security, and to be urged to take suitable precautions against such occult errors. 'It is to the observer,' says Gauss[1], 'that belongs the task of carefully removing the causes of constant errors,' and this is quite true when the error is absolutely constant. When we have made a number of determinations with a certain apparatus or method of measurement, there is a great advantage in altering the arrangement, or even devising some entirely different method of getting estimates of the same quantity. The reason obviously consists in the improbability that exactly the same constant error will affect two or more different methods of experiment. If a discrepancy is found to exist, we shall at least be aware of the existence of error, and can take measures for finding in which way it lies. If we can try a considerable number of methods, the probability becomes considerable that errors constant in one method will be balanced or nearly so by errors of an opposite effect in the others. Suppose that there be three different methods each affected by an error of equal amount. The probability that this error will in all fall in the same direction is only $\frac{1}{4}$; and with four methods similarly $\frac{1}{8}$. If each method be affected, as is always the case by several independent sources of error, the probability becomes very great that in the mean result of all the methods some of the errors will partially compensate the others. In this case, as in all others, when human foresight and vigilance has exhausted itself, we must trust the theory of probability.

In the determination of a zero point, of the magnitude of the fundamental standards of time and space, in the personal equation of an astronomical observer, we have instances of such fixed errors; but as a general rule a change of procedure is likely to reverse the character of the error, and many instances may be given of the value of this precaution.

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[1] Gauss, translated by Bertrand, p. 25.

If we measure over and over again the same angular magnitude by the same divided circle, maintained in exactly the same position, it is evident that the same mark in the circle will be the criterion in each case, and any error in the position of that mark will equally affect all our results. But if in each measurement we use a different part of the circle, a new mark will come into use, and as the error of each mark can hardly be in the same direction, the average result will be nearly free from errors of division. It will be still better to use more than one divided circle.

Even when we have no clear perception of the points of our apparatus at which fixed error is likely to enter, we may with advantage vary the construction of our apparatus with the hope that we shall accidentally detect some latent imperfection. Baily's purpose in repeating the experiments of Michell and Cavendish on the density of the earth, was not merely to follow the same course and verify the previous numbers, but to try whether variations in the size and substance of the attracting balls, the mode of suspension, the temperature of the surrounding air, &c., would yield different results. He performed no less than 62 distinct series, comprising 2153 experiments, and he carefully classified and discussed the results so as to disclose the utmost differences. Again, in experimenting upon the resistance of the air to the motion of a pendulum, Baily employed no less than 80 pendulums of various forms and materials, in order to ascertain exactly upon what conditions the resistance depends. Regnault, in his exact researches upon the dilatation of gases made arbitrary changes in the magnitude of parts of his apparatus. He thinks that if, in spite of such modification the results are unchanged, the errors are probably of inconsiderable amount[u]; but in reality it is always possible, and usually likely, that we overlook sources of error which a future generation will detect. Thus the pendulum experiments of Baily and Sabine were directed to ascertain the nature and amount of a correction for air resistance, which had been entirely misunderstood in the experiments upon which was founded the definition of the standard yard, by means of the seconds pendulum in the Act of 5th George IV. c. 74. It has already been mentioned that a considerable error was discovered in the determination of the standard metre as the ten-millionth part of the distance from the pole to the equator (p. 368).

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[u] Jamin, 'Cours de Physique,' vol. ii. p. 60.

We shall return in the second volume to the further consideration of the methods by which we may as far as possible secure ourselves against permanent and undetected sources of error. In the meantime, having completed the consideration of the special methods requisite for treating quantitative phenomena, we must return to our principal subject, and endeavour to trace out the course by which the physicist, from observation and experiment, collects the materials of natural knowledge, and then proceeds by hypothesis and inverse calculation to educe from them the laws of nature.

END OF THE FIRST VOLUME.

BOOK IV.

INDUCTIVE INVESTIGATION.

CHAPTER XVIII.

OBSERVATION.

ALL knowledge proceeds originally from experience. Using the name in a wide sense we may say that experience comprehends all that we *feel*, externally or internally—the aggregate of the impressions which we receive through the various apertures of perception— the aggregate consequently of what is in the mind, except so far as some portions of knowledge may be the reasoned equivalents of other portions. As the word experience implies[a], we *go through* much in life, and the impressions gathered intentionally or unintentionally afford the materials from which the active powers of the mind evolve science.

No small part of the experience actually employed in science is acquired without any distinct purpose. We cannot use the eyes without gathering some facts which may prove useful. Every great branch of science has generally taken its first rise from an accidental observation. Erasmus Bartholinus thus first discovered double refraction in Iceland spar; Galvani noticed the twitching of a frog's leg; Oken was struck by the form of a vertebra; Malus unintentionally examined with a double refracting substance light reflected from distant windows; and Sir John Herschel's attention was drawn to the peculiar appearance of a solution of quinine sulphate. In earlier times there must have been some one who first noticed the strange behaviour of a loadstone, or the unaccountable motions produced by amber. As a general rule we shall not know in what direction to look for a great body of phenomena widely different from those familiar to us. Chance then must give us the starting point; but one accidental observation well used may lead us to make thousands of observations in an intentional and organized manner, and thus a science may be gradually worked out from the smallest opening.

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[a] Max Müller's 'Lectures on Language,' vol. ii. p. 73.

Distinction of Observation and Experiment.

It is usual to say that the two modes of experience are Observation and Experiment. When we merely note and record the phenomena which occur around us in the ordinary course of nature we are said *to observe.* When we change the course of nature by the intervention of our will and muscular powers, and thus produce unusual combinations and conditions of phenomena, we are said *to experiment.* Sir John Herschel has justly remarked[b] that we might properly call these two modes of experience *passive and active observation.* In both cases we must certainly employ our senses to observe, and an experiment differs from a mere observation in the fact that we more or less influence the character of the events which we observe. Experiment is thus observation *plus* alteration of conditions.

It may readily be seen that we pass upwards by insensible gradations from pure observation to determinate experiment. When the earliest astronomers simply noticed the ordinary motions of the sun, moon, and planets upon the face of the starry heavens, they were pure observers. But astronomers now select precise times and places for important observations of stellar parallax, or the transits of planets. They make the earth's orbit the basis of a well arranged natural experiment, as it were, and take well considered advantage of motions which they cannot control. Meteorology might seem to be a science of pure observation, because we cannot possibly govern the changes of weather which we record. Nevertheless we may ascend mountains, or rise in balloons, like Gay-Lussac and Glaisher, and may thus so vary the points of observation as to render our procedure experimental. We are wholly unable either to produce or prevent earth currents of electricity, but when we construct long lines of telegraphic wires, we gather such strong currents during periods of disturbance as to render them capable of easy observation.

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[b] 'Preliminary Discourse on the Study of Natural Philosophy,' p. 77.

The most well arranged and assiduous systems of observation, however, would fail to give us a large part of the facts which we now possess. Many of the processes which are continually going on in nature may be so slow and gentle in operation that they would for ever escape our powers of observation. Lavoisier remarked that the decomposition of water must have been constantly proceeding in nature, although its possibility was unknown till his time[c]. No substance is wholly destitute of magnetic or diamagnetic powers; but it required all the experimental skill of Faraday to prove that iron, and a few other metals had no monopoly of these powers. Passive and accidental observation long ago impressed upon men's minds the phenomena of lightning, and the attractive properties of amber. Experiment only could have shown that phenomena so diverse in magnitude and character were manifestations of the one same agent. To observe with accuracy and convenience we must have agents under our control, so as to raise or lower their intensity, to stop or set them in action at will. Just as Smeaton found it requisite to create an artificial and governable supply of wind for his investigation of windmills, so we must have constant or governable supplies of light, heat, electricity, muscular force, or whatever other agents we are examining.

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[c] Lavoisier's 'Elements of Chemistry,' transl. by Kerr, 3rd ed. p. 148.

It is hardly needful to point out too that on the earth's surface we live under nearly uniform conditions of gravity, temperature, and atmospheric pressure, so that if we are to extend our inferences to other parts of the universe where conditions may be widely different, we must be prepared to imitate those conditions on a small scale here. We must have intensely high and low temperatures; we must vary the density of gases from approximate vacuum upwards; we must subject liquids and solids to pressures or strains of almost unlimited amount.

Mental Conditions of Correct Observation.

Every observation must in a certain sense be true, for the observing and recording of an event is in itself an event. But before we proceed to deal with the supposed meaning of the record, and draw inferences concerning the course of nature, we must take care to ascertain that the position and feelings of the observer are not to a great extent the phenomena recorded. The mind of man, as Francis Bacon said, is like an uneven mirror, and does not reflect the events of nature without distortion. We need not take notice of intentionally false observations, nor of mistakes arising from defective memory, deficient light, and so forth. Even where the utmost intentional fidelity and care are used in observing and recording, tendencies to error exist, and fallacious opinions arise in consequence.

It is exceedingly rare to find persons who can with perfect fairness estimate and register facts for and against their own peculiar views and theories. Among uncultivated observers the tendency to remark favourable and forget unfavourable events is so great, that no reliance can be placed upon their supposed observations. Thus arises the enduring fallacy that the changes of the weather coincide in some way or other with the changes of the moon, although exact and impartial registers give no countenance to the fact. The whole race of prophets and quacks live upon the overwhelming effect of one success, compared with hundreds of failures which are unmentioned and forgotten. As Bacon says, 'Men mark when they hit, and never mark when they miss.' We should do well to bear in mind the ancient story, quoted by Bacon, of one who in Pagan times was shown a temple with a picture of all the persons who had been saved from shipwreck, after paying their vows. When asked whether he did not now acknowledge the power of the gods, 'Aye,' he answered; 'but where are they painted that were drowned after their vows?'

If indeed we could estimate the amount of *bias* existing in any particular observations, it might be treated like one of the forces of the problem, and the true course of external nature might still be rendered apparent. But the feelings of an observer are usually too indeterminate, so that whenever there is reason to suspect any considerable amount of bias, rejection is the only safe course. As regards facts casually registered in past times, the capacities and impartiality of the observer are so little known that we should spare no pains to replace these statements by a new appeal to nature. An indiscriminate medley of truth and absurdity, such as Francis Bacon has collected in his

Natural History, is wholly unsuited to the purposes of science. But of course when records relate to past events like eclipses, conjunctions, meteoric phenomena, earthquakes, volcanic eruptions, changes of sea margins, the existence of now extinct animals, the migrations of tribes, floods, &c., we must depend upon traditions or records, however unsatisfactory, and must endeavour to verify the statements by the comparison of independent records.

When extensive series of observations have to be made, as in astronomical, meteorological, or magnetical observatories, trigonometrical surveys, and extensive chemical or physical researches, it is an advantage that the numerical estimations and records should be executed by assistants who are not interested in, and are perhaps unaware of, the expected results. The record is thus rendered perfectly impartial. It may even be desirable that those who perform the purely routine work of measurement and computation should be unacquainted with the principles of the subject. The great table of logarithms of the French Revolutionary Government was worked out by a staff of sixty or eighty computers, most of whom were acquainted only with the rules of arithmetic, and worked under the direction of skilled mathematicians; yet their calculations were usually found more correct than those of persons more deeply versed in mathematics[d]. In the Indian Ordnance Survey the actual measurers have been selected so that they shall not have sufficient skill to falsify their results without detection.

Both passive observation and experimentation must, however, be generally conducted by persons who know for what they are to look. It is only when excited and guided by the hope of verifying a theory that the observer will notice many of the most important points; and, where the work is not of a routine character, no assistants can supersede the mind-directed observations of the philosopher. Thus the successful investigator must combine diverse qualities; he must have clear notions of the result he expects, and confidence in the truth of his theories, and yet he must have that candour and flexibility of mind, which enable him to accept unfavourable results and abandon mistaken views.

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[d] Babbage, 'Economy of Manufactures,' p. 194.

Instrumental and Sensual Conditions of Correct Observation.

In every observation one or more of the senses must be employed, and we should ever bear in mind that the extent of our knowledge may be limited by the power of the sense concerned. What we learn of the world only forms the lower limit of what is to be learned, and, for all that we can tell, the processes of nature may indefinitely surpass in variety and complexity those which are capable of coming within our means of observation. In some cases inference from observed phenomena may make us indirectly aware of what cannot be directly felt, but we can never be sure that we thus acquire any appreciable fraction of the knowledge that might be acquired.

It is a strange reflection that space may be filled with dark wandering stars, whose existence could not have yet become in any way known to us. The planets have already cooled so far as to be no longer luminous, and it may well be that other stellar bodies of various size have fallen into the same condition. From the consideration, indeed, of variable and extinguished stars, Laplace inferred that there probably exist opaque bodies as great and perhaps as numerous as those we see[c]. Some of these dark stars might ultimately become known to us, either by reflecting light, or more probably by their gravitating effects upon luminous stars. Thus if one member of a double star were dark, we could readily detect its existence, and even estimate its size, position, and motions, by observing those of its visible companion. It was a favourite notion of Huyghens that there may exist stars and vast universes so distant that their light has never yet had time to reach our eyes; and we must also bear in mind that light may possibly suffer slow extinction in space, so that there is more than one way in which an absolute limit to the powers of telescopic discovery may exist.

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[c] 'System of the World,' translated by Harte, vol. ii. p. 335.

There are natural limits again to the power of our senses in detecting undulations of various kinds. It is commonly said that vibrations of less than sixteen strokes or more than 38,000 strokes per second are not audible as sound; and as some ears actually do hear sounds of much higher pitch, even two octaves higher than what other ears can detect, it is exceedingly probable that there are incessant vibrations which we cannot call sound because they are never heard. Insects may possibly communicate by such acute sounds, constituting a language inaudible and inscrutable to us; and the remarkable agreement apparent among bodies of ants or bees might thus perhaps be explained. Nay, as Fontenelle long ago suggested in his scientific romance, there may exist unlimited numbers of senses or modes of perception which we can never feel, though Darwin's theory would render it probable that any useful means of knowledge in an ancestor would be developed and improved in the descendants. We might doubtless have been endowed with a sense capable of feeling electric phenomena with acuteness, so that the positive or negative state of charge of a body could be at once estimated. The absence of such a sense is probably due to its comparative uselessness.

Heat undulations are subject to the same considerations.

It is now apparent that what we call light is the affection of the eye by certain vibrations, the less rapid of which are invisible and constitute the dark rays of radiant heat, in detecting which we must substitute the thermometer or the thermopile for the eye. At the other end of the spectrum, again, the ultra-violet rays are invisible, and only indirectly brought to our knowledge in the phenomena of fluorescence or photo-chemical action. There is no reason to believe that at either end of the spectrum an absolute limit has yet been reached.

Just as our knowledge of the stellar universe is limited by the power of the telescope and other conditions, so our knowledge of the minute world has its limit in the powers and optical conditions of the microscope. There was a time when it would have been a reasonable induction that vegetables were motionless, and animals alone endowed with power of locomotion. We are astonished to discover by the microscope that minute plants are if anything more active than minute animals. We even find that mineral substances seem to lose their inactive character and dance about with incessant motion when reduced to sufficiently minute particles, at least when suspended in a non-conducting medium[f]. Microscopists will meet a natural limit to their means of observation when the minuteness of the objects examined becomes comparable to the length of light undulations, and the extreme difficulty already encountered in determining the forms of minute marks on Diatoms appears to be due to this cause.

Of the errors likely to arise in estimating quantities by the senses I have already spoken (vol. i. p. 320), but there are some cases in which we actually see things different from what they are. A jet of water often appears to be a continuous thread, when it is really a wonderfully organized succession of small and large drops, oscillating in form. The drops fall so rapidly that their impressions upon the eye run into each other, and in order to see the separate drops we require some device for giving an instantaneous view, such as illumination by the electric spark, or the use of the revolving disc called the phenakistiscope.

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[f] Jevons, 'Proceedings of the Literary and Philosophical Society of Manchester,' 25th January, 1870, vol. ix. p. 78.

One insuperable limit to our powers of observation arises from the impossibility of following and identifying the ultimate atoms of matter. One atom of oxygen is probably undistinguishable from another atom; only by keeping a certain volume of oxygen safely enclosed in a bottle can we assure ourselves of its identity; allow it to mix with other oxygen, and we have lost all power of identification. Accordingly we seem to have no means of directly proving that every gas is in a constant state of diffusion of every part into every part. We can only infer this to be the condition from observing the behaviour of distinct gases which we can distinguish in their course, and by reasoning on the grounds of molecular theory[g].

External Conditions of Correct Observation.

Before we proceed to draw inferences from any series of recorded facts, we must take great care to ascertain perfectly, if possible, the external conditions under which the facts are brought to our notice. Not only may the observing mind be prejudiced and the senses defective, but there may be circumstances which cause one kind of event to come more frequently to our notice than another. The comparative numbers of events or objects of different kinds existing may in any degree differ from the numbers which we are able to record. This difference must if possible be taken into account before we make any inferences.

[g] Maxwell, 'Theory of Heat,' p. 301.

There long appeared to be a strong presumption that all comets moved in elliptic orbits, because no comet had been proved to move in any other kind of path. The theory of gravitation admitted of the existence of comets moving in hyperbolic orbits, and the question arose whether they were really non-existant or were only beyond the bounds of easy observation. From reasonable suppositions Laplace calculated that the probability was at least 6000 to 1 against a comet which comes within the planetary system sufficiently to be visible at the earth's surface, presenting an orbit which could be discriminated from a very elongated ellipse or parabola, in the part of its orbit within the reach of our telescopes[h]. In short, the chances are very much in favour of our seeing elliptic rather than hyperbolic comets. Laplace's views have been confirmed by the discovery of six hyperbolic comets, which appeared in the years 1729, 1771, 1774, 1818, 1840, and 1843[i], and, as only about 800 comets altogether have been recorded, the proportion of hyperbolic ones is quite as large as should be expected. Some remarkable speculations have recently been published by Mr. A. S. Davies, as to the probable character of the orbits of comets, which, after moving freely through space, become attached to this planetary system[k].

When we attempt to estimate the numbers of objects which may have existed, we must make large allowances for the limited sphere of our observations. Thus probably not more than 4000 or 5000 comets have been seen in historical times, but making allowance for the absence of observers in the southern hemisphere, and for the small probability that we see any considerable fraction of those which are in the neighbourhood of our system, we must accept Kepler's opinion, that there are more comets in the regions of space than fishes in the depths of the ocean. When like calculations are made concerning the numbers of meteors visible to us, it is astonishing to find that the number of meteors entering the earth's atmosphere in every twenty-four hours is probably not less than 400,000,000, of which 13,000 exist in every portion of space equal to that filled by the earth's globe.

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[h] Laplace, 'Essai Philosophique,' p. 59. Todhunter's 'History,' pp. 491–94.

[i] Chamber's 'Astronomy,' 1st ed. p. 203.

[k] 'Philosophical Magazine,' 4th Series, vol. xl. p. 190; vol. xli. p 44.

Most serious fallacies may arise from overlooking the inevitable conditions under which the records of past events are brought to our notice. Thus it is only the durable objects manufactured by former races of men, such as flint implements, which can have come to our notice as a general rule. The comparative abundance of iron and bronze articles used by an ancient nation must not be supposed to be coincident with their comparative abundance in our museums, because bronze is far the more durable. There is always a prevailing fallacy that our ancestors built more strongly than we do, arising from the fact that the more fragile structures have long since crumbled away. It is thus that we have few or no relics of the habitations of the poorer classes among the Greeks or Romans, or in fact of any past race; for the temples, tombs, public buildings and mansions of the wealthier classes alone endure. There is an indefinite expanse of past events necessarily lost to us for ever, and we must generally look upon records or relics as exceptional in their character.

Exactly the same considerations apply to geological relics. We could not generally expect that animals would be preserved, unless as regards the bones, shells, strong integuments, or other hard and durable parts. All the infusoria and animals devoid of mineral framework must probably have perished entirely, distilled perhaps into oils. It has been pointed out that the peculiar character of some extinct floras may be due to the unequal preservation of different families of plants. By various accidents, however, we may gain glimpses of a world that is usually lost to us—as by insects embedded in amber, the great mammoth preserved in ice, mummies, casts in solid material like that of the Roman soldier at Pompeii, and so forth.

We should also remember, that just as there may be conjunctions of the heavenly bodies that can have happened only once or twice in the period of history, so remarkable terrestrial conjunctions may take place. Great storms, earthquakes, volcanic eruptions, landslips, floods, irruptions of the sea may, or rather must, have occurred, events of such unusual magnitude and such extreme rarity that we can neither expect to witness them nor readily to comprehend their effects. It is a great advantage of the study of probabilities, as Laplace himself remarked, to make us mistrust the extent of our knowledge, and pay proper regard to the probability that events would come within the sphere of our observations.

Apparent Sequence of Events.

De Morgan has excellently pointed out[1] that there are no less than four modes in which one event may seem to follow or be connected with another, without being really so. These involve mental, sensual, and external causes of error, and I will briefly state and illustrate them.

Instead of A causing B, it may be *our perception of A that causes B.* Thus it is that prophecies, presentiments, and the devices of sorcery and witchcraft often work their own ends. A man dies on the day which he has always regarded as his last, from his own fears of the day. An incantation effects its purpose, because care is taken to frighten the intended victim, by letting him know his fate[m]. In all such cases the mental condition is the cause of apparent coincidence.

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[1] 'Essay on Probabilities,' Cabinet Cyclopædia, p. 121.

In a second class of cases, *the event A may make our perception of B follow, which would otherwise happen without being perceived.* Thus it was seriously believed as the result of investigation that more comets appeared in hot than cold summers. No account was taken of the fact that hot summers would be comparatively cloudless, and afford better opportunities for the discovery of comets[n]. Here the disturbing condition is of a purely external character. Certain ancient philosophers held that the moon's rays were cold-producing, mistaking the cold caused by radiation into space for an effect of the moon, which becomes visible at the time when the absence of clouds permits radiation to proceed.

In a third class of cases, *our perception of A may make our perception of B follow.* The event B may be constantly happening, but our attention may not be drawn to it except by our observing A. This case seems to be illustrated by the fallacy of the moon's influence on clouds. The origin of this fallacy is somewhat complicated. In the first place, when the sky is densely clouded the moon would not be visible at all; it would be necessary for us to see the full moon in order that our attention should be strongly drawn to the fact, and this would happen most often on those nights when the sky was cloudless. Mr. W. Ellis[o], moreover, has ingeniously pointed out that there is a general tendency for clouds to disperse at the commencement of night, which is the time when the full moon rises. Thus the change of the sky and the rise of the full moon are likely to attract attention mutually, and the coincidence in time suggests the relation of cause and effect. Mr. Ellis proves from the results of observations at the Greenwich Observatory that the moon possesses no appreciable power of the kind supposed, and yet it is remarkable that so acute and sound an observer as the late Sir John Herschel was convinced of the connection. In his 'Results of Observations at the Cape of Good Hope'[p], he mentions many evenings when a full moon occurred with a peculiarly clear sky.

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[m] Lubbock, 'Origin of Civilization,' p. 148.

[n] De Morgan's 'Essay,' p. 123.

[o] 'Philosophical Magazine,' 4th Series (1867), vol. xxxiv. p. 64.

There is yet a fourth class of cases, in which *B is really the antecedent event, but our perception of A, which is a consequence of B, may be necessary to bring about our perception of B.* There can be no doubt, for instance, that upward and downward currents are continually circulating in the lowest stratum of the atmosphere during the day-time; but owing to the transparency of the atmosphere we have no evidence of their existence until we perceive cumulous clouds, which are the consequence of such currents. In like manner an interfiltration of bodies of air in the higher parts of the atmosphere is probably in nearly constant progress, but unless threads of cirrous cloud indicate these motions we remain wholly ignorant of their occurrence[q]. The highest strata of the atmosphere are wholly imperceptible to us, except when rendered luminous by auroral currents of electricity, or by the passage of meteoric stones.

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[p] See 'Notes to Measures of Double Stars,' 1204, 1336, 1477, 1686, 1786, 1816, 1835, 1929, 2081, 2186, pp. 265, &c. See also Herschel's 'Familiar Lectures on Scientific Subjects, p. 147, and 'Outlines of Astronomy,' 7th ed. p. 285.

[q] Jevons, 'On the Cirrous Form of Cloud,' Philosophical Magazine, July, 1857, 4th Series, vol. xiv. p. 22.

There are many phenomena in meteorology and other similar sciences, in which some occurrences depend on others for their visibility. Thus in estimating the comparative numbers of meteors seen in different months of the year, it is essential to take account of the varying frequency of cloudy weather—or else of the different duration of the daylight which hides all but the most splendid meteors. Observations of the comparative frequency of various kinds of clouds will be complicated by the fact that dense rain clouds necessarily hide those more delicate cirrous clouds which appear in the higher parts of the atmosphere. Most of the visible phenomena of comets probably arise from some substance which, existing previously invisible, becomes condensed or electrified suddenly into a visible form. Sir John Herschel attempted to explain the production of comet tails in this manner by evaporation and condensation[r].

Negative Arguments founded on the Non-observation of Phenomena.

From what has been suggested in preceding sections, it will plainly appear that the non-observation of a phenomenon is not generally to be taken as proving its non-occurrence. As there are sounds which we cannot hear, rays of light which we cannot feel, indefinite multitudes of worlds which we cannot see, and infinite myriads of minute organisms of which not the most powerful microscope can give us a view, we must as a general rule interpret our experience in an affirmative sense only. Accordingly when inferences have been drawn from the non-occurrence of particular facts or objects, more extended and careful examination has often proved their falsity. Not many years since it was quite a well credited conclusion in geology that no remains of man were found in connexion with those of extinct animals, or in any deposit not actually at present in course of formation. Even Babbage accepted this conclusion as strongly confirmatory of the Mosaic accounts[a]. But when the opinion was yet universally held, flint implements had been found disproving any such conclusion, and overwhelming evidence of man's long continued existence has since been found. At the end of the last century when Herschel had searched the heavens with his powerful telescopes, there seemed little probability that planets yet remained unseen within the orbit of Jupiter. But on the first day of this century such an opinion was overturned by the discovery of Ceres, and more than a hundred other small planets have since been added to the lists of the planetary system.

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[r] 'Astronomy,' 4th ed. p. 358.

The discovery of the Eozoon Canadense in strata of much greater age than any previously known to contain organic remains, has given a severe shock to many groundless opinions concerning the origin of organic forms; and the oceanic dredging expeditions, under Dr. Carpenter and Professor Wyville Thompson, have further disconcerted geologists by disclosing the continued existence of forms long supposed to be extinct. These and many other cases which might be quoted show the extremely unsafe character of negative inductions.

It must not be supposed that negative arguments are of no force and value. The earth's surface, for instance, has been sufficiently searched to render it highly improbable that any terrestrial animals of the size of a camel remain to be discovered. It is believed that no new large animal has been encountered in the last eighteen or twenty centuries[t], and the probability that if existent they would have been seen, increases the probability that they do not exist. We may with somewhat less confidence discredit the existence of any large unrecognised fish, or sea animals, such as the alleged sea-serpent. But as we descend to forms of smaller size negative evidence loses weight from the less probability of our seeing smaller objects. Even the strong induction in favour of the four-fold division of the animal kingdom into Vertebrata, Annulosa, Mollusca, and Cœlenterata, may break down by the discovery of intermediate or anomalous forms. As civilisation spreads over the surface of the earth, and unexplored tracts are gradually diminished, negative conclusions will increase in force; but we require to learn much yet concerning the depths of the ocean, almost wholly unexamined as they are, and covering three-fourths of the earth's surface.

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[a] Babbage, 'Ninth Bridgewater Treatise,' p. 67.

In geology there are a number of assertions to which considerable probability attaches on account of the large extent of the investigations already made, as, for instance, that true coal is found only in rocks of a particular geological epoch; that gold occurs in secondary and tertiary strata only in exceedingly small quantities[u], probably derived from the disintegration of earlier rocks.

In natural history negative conclusions are exceedingly treacherous and unsatisfactory. The utmost patience will not enable a microscopist or the observer of any living thing to watch the behaviour of the organism under all circumstances continuously for any great length of time. There is always a chance therefore that the critical act or change may take place when the observer's eyes are withdrawn. This certainly happens in some cases; for though the fertilization of orchids by agency of insects is proved as well as any fact in natural history, Mr. Darwin has never been able by the closest watching to detect an insect in the performance of the operation. Mr. Darwin has himself, indeed, adopted one conclusion on purely negative evidence, namely that the Orchis pyramidalis and certain other orchidaceous flowers secrete no nectar. But his caution and unwearying patience in verifying the conclusion give an impressive lesson to the observer. For twenty-three consecutive days, as he tells us, he examined flowers, in all states of the weather, at all hours, in various localities. As the secretion in other flowers sometimes rapidly takes place and might happen at early dawn, that inconvenient hour of observation was specially adopted. Flowers of different ages were subjected to irritating vapours, to water, and every condition likely to bring on the secretion; and only after the invariable failure of this exhaustive inquiry was the barrenness of the nectaries assumed to be proved[x].

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[t] Cuvier's 'Essay on the Theory of the Earth,' translation, p. 61, &c.

[u] Murchison's 'Siluria,' 1st ed. p. 432.

In order that a negative argument founded on the non-observation of an object shall have any considerable force, it must be shown to be probable that the object if existent would have been observed, and it is this probability which defines the value of the negative conclusion. The failure of astronomers to see the planet Vulcan, supposed by some to exist within Mercury's orbit, is no sufficient disproof of its existence. Similarly it would be very difficult, or even impossible, to disprove the existence of a second satellite of small size revolving round the earth. But if any person make a particular assertion, assigning place and time, then observation will either prove or disprove the alleged fact. Thus if it is true that when a French observer professed to have seen a planet on the sun's face, an observer in Brazil was carefully scrutinizing the sun and failed to see it, we have a conclusive negative proof[y]. On this account, as it has been well said, false facts in science are more mischievous than false theories. A false theory is open to every person's criticism, and is ever liable to be judged by its accordance with facts. But a false or grossly erroneous assertion of a fact often stands in the way of science for a long time, because it may be extremely difficult or even impossible to prove the falsity of what has been once recorded.

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[x] Darwin's 'Fertilization of Orchids,' p. 48.

In other sciences the force of a negative argument will often depend upon the number of possible alternatives which may exist. Thus it was long believed that the character or quality of a musical sound, as distinguished from its pitch must depend upon the form of the undulation, because no other cause of it had ever been suggested or was apparently possible. The truth of the conclusion was proved by Helmholtz, who applied a microscope to luminous points attached to the strings of various instruments, and thus actually observed the different modes of undulation[z].

In mathematics negative inductive arguments have seldom much force, because the possible forms of expression, or the possible combinations of lines and circles in geometry are quite unlimited in number. An enormous number of attempts were made to trisect the angle by the ordinary methods of Euclid's geometry, but their invariable failure did not establish the impossibility of the task. This was shown in a totally different manner, by proving that the problem involves an irreducible cubic equation to which there could be no corresponding plain geometrical solution[a]. This is a case of *reductio ad absurdum*, a form of argument of a totally different character. Similarly no number of failures to obtain a general solution of equations of the fifth degree would establish the impossibility of the task, but in an indirect mode, equivalent to a *reductio ad absurdum*, the impossibility is considered to be proved[b].

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[y] Chambers's 'Astronomy,' 1st ed. p. 31.

[z] 'Théorie Physiologique de la Musique', Paris, 1868, p. 113.

[a] Peacock, 'Algebra,' vol. ii. p. 344.

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[b] Peacock, 'Algebra,' vol. ii. p. 359. Serret, 'Algèbra Supérieure,' 2nd ed. p. 289.

CHAPTER XIX.

EXPERIMENT.

We now come to consider the great facilities which we enjoy for examining the possible combinations of properties and phenomena when objects are within our reach and capable of manipulation. We are said to *experiment*, when we bring substances together under various conditions of temperature, pressure, electric disturbance, molecular attraction, &c., and then record the changes observed.

If we denote by A a certain group of antecedent conditions, and by X a certain series of subsequent phenomena, our object will usually be to ascertain a law of the form $A = AX$, the meaning of which is that where A is X will happen, and we may sometimes rise to the still simpler and higher law $A = X$, meaning that where A is, and only where A is, X will happen (see vol. i. pp. 146, 149.)

The great object of the art of experiment is to ascertain exactly those circumstances or conditions which are requisite for the happening of any event X. Now the circumstances which might be enumerated as present in the very simplest experiment are very numerous, in fact almost infinite. Rub two sticks together and consider what would be an exhaustive statement of the conditions. There are the form, hardness, organic structure, and all the chemical qualities of the wood; the pressure and velocity of the rubbing; the temperature, pressure, and all the chemical qualities of the surrounding air; the proximity of the earth with its attractive and electric properties; the temperature and other powers of the persons producing motion; the radiation from the sun, and to and from the sky; the electric excitement possibly existing in any overhanging cloud; even the positions of the heavenly bodies must be mentioned. Now on *à priori* grounds it is unsafe to assume that any one of these circumstances is without effect, and it is only on the results of experience that we can finally single out those precise conditions from which the observed heat of friction proceeds.

The great method of experiment consists in removing, one at a time, each of those conditions which may be imagined to have an influence on the result. Our object in the experiment of rubbing sticks is to discover the exact circumstances under which heat appears. Now the presence of air may be requisite; therefore prepare a vacuum, and rub the sticks in every respect as before, except that it is done *in vacuo.* If heat still appears we may say that air is not, in the presence of the other circumstances, a requisite condition. The conduction of heat from neighbouring bodies may be a condition. Prevent this by making all the surrounding bodies ice cold, which is practically what Davy aimed at in rubbing two pieces of ice together. If heat still appears we have eliminated another condition, and so we may go on until it becomes apparent that the expenditure of energy in the friction of two bodies is the sole condition of the production of heat.

The great difficulty of experiment arises from the fact that we must not assume an independence to exist among the conditions. Thus previous to experiment we have no right to say that the rubbing of two sticks will produce heat in the same way when air is absent as before. We may have heat produced in one way when air is present, and in another when air is absent. The inquiry branches out into two lines, and we ought to try in both cases whether cutting off a supply of heat by conduction prevents its evolution in friction. Now the same branching out of the inquiry occurs with regard to every circumstance which enters into the experiment. Regarding only four circumstances, say A, B, C, D, we ought to test not only the combinations—

ABCD, ABC*d*, AB*c*D, A*b*CD, *a*BCD,

but we ought really to go through the whole of the combinations given in the fifth column of the Logical Abecedarium. The effect of the absence of each condition should be tried both in the presence and absence of every other condition, and every variety of selection of those conditions. Perfect and exhaustive experimentation would, in short, consist in examining natural phenomena in all their possible combinations and registering all relations between conditions and results which are found capable of existence. Experimentation would thus resemble the exclusion of contradictory combinations carried out in the Indirect Method of Inference (chapter vi. vol. i. p. 95), except that the exclusion of any combination is grounded not on prior logical premises, but on *à posteriori* results of actual trial.

The reader will readily perceive, however, that such exhaustive investigation is practically impossible, because the number of requisite experiments would be immensely great. Four circumstances only would require sixteen experiments; twelve circumstances would require 4096, and the number increases as the powers of two. The result is that the experimenter has to fall back upon his own tact and experience in selecting those variations which are most likely to yield him significant facts. It is at this point that logical rules and forms begin to fail in giving aid. The logical rule is—Try all possible combinations; but this being impracticable, the experimentalist necessarily abandons strict logical method, and trusts to his own insight. Analogy, as we shall afterwards see, gives some assistance, and attention will probably be concentrated on those kinds of conditions which have been found important in like cases. But we are now entirely in the region of probability, and the experimenter, while he is confidently pursuing what he thinks the right clue, may be entirely overlooking the one condition whose importance has been hitherto unsuspected. It is an impressive lesson, for instance, that Newton pursued all his exquisite researches on the spectrum unsuspicious of the fact that if he reduced the hole in the shutter to a narrow slit, all the mysteries of the bright and dark lines were within his grasp, provided of course that his prisms were sufficiently good to define the rays. In a similar manner we know not what slight alteration in the most familiar experiments may not open the way to realms of new discovery.

Many additional practical difficulties encumber the progress of the physicist. It is often impossible to alter one condition without altering others at the same time; and thus we may not get the pure effect of the condition in question. Some conditions may be absolutely incapable of alteration; others may be with great difficulty, or only in a certain degree, removable. A very treacherous source of error is the existence of unsuspected conditions, which we of course cannot remove except by accident. These difficulties we will shortly consider in succession.

It is often beautiful to observe how the alteration of a single circumstance conclusively explains a phenomenon. An excellent instance is found in Faraday's investigation of the behaviour of Lycopodium spores scattered on a vibrating plate. It was observed that these minute spores collected together at the points of greatest motion, whereas sand and all heavy particles collect at the nodes, where motion is least. But it happily occurred to Faraday to try the experiment in the exhausted receiver of an air-pump, and it was then found that the light powder behaved exactly like heavy powder. A conclusive proof was thus obtained that the presence of air was the condition of importance, doubtless because it was thrown into eddies by the motion of the plate, and thus carried the Lycopodium to the points of greatest agitation. Sand was too heavy to be thus carried by the air.

Exclusion of Indifferent Circumstances.

From what has been already said it will be apparent that in the investigation of any new phenomenon the detection and exclusion of indifferent circumstances is a work of great importance, because it allows the concentration of attention upon circumstances which may contain the principal condition. There will always be a multitude of things which we are only too ready to neglect, but many beautiful instances may be given where all the most obvious circumstances have been shown to have no part in the production of a phenomenon. Every person would suppose that the peculiar colours of mother-of-pearl were due to the chemical qualities of the substance. Much trouble might have been spent in following out that notion by comparing the chemical qualities of various iridescent substances. But Brewster accidentally took an impression from a piece of mother-of-pearl in a cement of resin and bees'-wax, and finding the colours repeated upon the surface of the wax, proceeded to take other impressions in balsam, fusible metal, lead, gum arabic, isinglass, &c., and always found the iridescent colours the same. He thus proved that the chemical nature is wholly a matter of indifference, and the form of the surface is the condition of such colours[a].

Nearly the same may be said of the colours exhibited by thin plates and films. The rings and lines of colour will be of the same character whatever may be the nature of the substance ; nay, a void space, such as a crack in glass, would produce them even though the air were withdrawn by an air-pump. The conditions are simply the existence of two reflecting surfaces separated by a very small space, though it should be added that the refractive index of the intervening substance has some influence on the exact nature of the colour produced at any point.

When a ray of light passes close to the edge of an opaque body, a portion of the light appears to be bent towards it, and produces coloured fringes within the shadow of the body. Newton attributed this inflexion of light to the attraction of the opaque body for the supposed particles of light, although he was aware that the nature of the surrounding medium, whether air or other pellucid substance, exercised no apparent influence on the phenomena. Gravesande proved however that the character of the fringes is exactly the same, whether the body be dense or rare, compound or elementary. A wire has exactly the same effect as a hair of the same thickness. Even the form of the obstructing edge was subsequently shown to be a matter of indifference by Fresnel, and the interference spectrum, or the spectrum seen when light passes through a fine grating is absolutely the same whatever be the form or chemical nature of the bars forming the grating. Thus it appears that the stoppage of a portion of a beam of light is the sole necessary condition for the diffraction or inflexion of light ; and the phenomenon is shown to bear no analogy to the reflection and refraction of light, in which the form and nature of the substance are all important.

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[a] 'Treatise on Optics,' by Sir D. Brewster, Cabinet Cyclopædia, p. 117.

It is interesting to observe how carefully Newton, in his researches on the spectrum, observed and proved the indifference of many circumstances by actual trial. He says[b]: 'Now the different magnitude of the hole in the window-shut, and different thickness of the prism where the rays passed through it, and different inclinations of the prism to the horizon, made no sensible changes in the length of the image. Neither did the different matter of the prisms make any: for in a vessel made of polished plates of glass cemented together in the shape of a prism, and filled with water, there is the like success of the experiment according to the quantity of the refraction.' But in the latter statement, as I shall afterwards remark (vol. ii. p. 42), Newton assumed an indifference which does not exist, and fell into an unfortunate mistake.

In the science of sound it is shown that the pitch of a sound depends solely upon the number of impulses in a second, and the material exciting those impulses is a matter of perfect indifference. Thus whatever medium, whether air or water, or any gas or liquid, be forced into the Siren, the sound produced is the same; and the material of which an organ-pipe is constructed does not at all affect the pitch of its sound.

In the science of statical electricity it is an important circumstance that the interior of a conducting body is a matter of indifference, resting in a neutral state, while the change is confined to the conducting surface. A hollow sphere takes exactly the same charge as a solid sphere of metal.

Some of Faraday's most elegant and successful researches were devoted to the exclusion of conditions which previous experimenters had thought essential for the production of electrical phenomena. Davy asserted that no known fluids, except such as contain water, could be made the medium of connexion between the poles of a battery; and some chemists believed that water was an essential agent in electro-chemical decomposition. Faraday gives abundant experiments to show that other fluids allow of electrolysis, and attributes the erroneous opinion to the very general use of water as a solvent, and its presence in most natural bodies[c]. It was, in fact, upon purely negative (vol. ii. p. 16) and weak evidence that the opinion had been founded.

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[b] 'Opticks,' 3rd edit. p. 25.

Many experimenters attributed peculiar and even mysterious powers to the poles of a battery, likening them to magnets, which, by their attractive powers, tear apart the elements of a substance. By a most beautiful series of experiments[d], Faraday proved conclusively that, on the contrary, the substance of the poles is of no importance, being merely the path through which the electric force reaches the liquid acted upon. Poles of water, charcoal, and many diverse substances, even air itself, produced similar results, or if the chemical nature of the pole entered at all into the question, it was as a disturbing agent.

It is a most essential part of the theory of gravitation that the proximity of other attracting particles is wholly without effect upon the attraction existing between any two molecules. Two pound weights weigh as much together as they do separately. Every pair of molecules in the world have, as it were, a private communication, apart from their relations to all other molecules. Another undoubted result of experience pointed out by Newton[e] is that the weight of a body does not in the least depend upon its form or texture. It may be added that the temperature, electric condition, pressure, state of motion, chemical qualities, and all other circumstances concerning matter, except its mass, are indifferent as regards its gravitating power.

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[c] 'Experimental Researches in Electricity,' vol. i. pp. 133, 134.

[d] Ibid. vol. i. pp. 127, 162, &c.

[e] 'Principia,' bk. iii. Prop. vi. Corollary i.

As natural science progresses, physicists gain a kind of insight and tact in judging what qualities of a substance are likely to be concerned in any class of phenomena. The physical astronomer treats matter in one point of view, the chemist in another, and the students of physical optics, sound, mechanics, electricity, &c., make a fair division of the qualities among them. But errors will arise if too much confidence be placed in this independence of various kinds of phenomena, so that it is desirable from time to time, especially when any unexplained discrepancies come into notice, to question the indifference which is assumed to exist, and to test its real existence by appropriate experiments.

Simplification of Experiments.

One of the most requisite precautions in experimentation is to vary only one circumstance at a time, and to maintain all other circumstances rigidly unchanged. There are two distinct reasons for this rule, the first and most obvious being that if we vary two conditions at a time, and find some effect, we cannot tell whether the effect is due to one or the other, or to both jointly. A second reason is that if no effect ensues we cannot safely conclude that either of them is indifferent; for the one may have neutralized the effect of the other. In our logical formulæ, A (B + *b*) is identical with A (see vol. i. p. 112), and B may be indifferently present or absent; but A (BC + *bc*) is not identical with A, and none of our logical processes enabled us to make the reduction.

If we want to prove that oxygen is necessary to life, we must not put a rabbit into a vessel from which the oxygen has been exhausted by a burning candle. We should then have not only an absence of oxygen, but an addition of carbonic acid, which may have been the destructive agent. For a similar reason Lavoisier avoided the use of atmospheric air in experiments on combustion, because air was not a simple substance, and the presence of nitrogen might impede or even alter the effect of oxygen. As Lavoisier expressly remarks[f], 'In performing experiments, it is a necessary principle, which ought never to be deviated from, that they be simplified as much as possible, and that every circumstance capable of rendering their results complicated be carefully removed.' It has also been well said by Cuvier[g] that the method of physical inquiry consists in isolating bodies, reducing them to their utmost simplicity, and in bringing each of their properties separately into action, either mentally or by experiment.

The electro-magnet has been of the utmost service in the investigation of the magnetic properties of matter, by allowing of the production or removal of a most powerful magnetic force without disturbing any of the other arrangements of the experiment. Many of Faraday's most valuable experiments would have been frustrated had it been necessary to introduce a heavy permanent magnet, which could not be suddenly moved without shaking the whole apparatus, disturbing the air, producing currents by differences of temperature, &c. The electro-magnet is perfectly under control, and its influence can be brought into action, reversed, or stopped by merely touching a button. Thus Faraday was enabled to prove the rotation of the plane of circular polarized light by the fact that a certain light ceased to be visible when the electric current of the magnet was cut off, and *vice versâ* the light appeared when the current was re-made. 'These phenomena,' he says, 'could be reversed at pleasure, and at any instant of time, and upon any occasion, showing a perfect dependence of cause and effect[h].'

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[f] Lavoisier's 'Chemistry,' translated by Kerr, p. 103.

[g] Cuvier's 'Animal Kingdom,' introduction, pp. 1, 2.

Another elegant experiment by Faraday illustrates the maintainance of similar conditions. He proved that liquids may conduct electricity when solids will not, by putting the poles of a battery in melted nitre, when a strong current was shown to exist by the galvanometer. But as soon as the nitre was allowed to solidify, the current ceased. Everything else remaining the same, the current existed when the nitre was liquid, and not when the nitre was solid[i].

It was Newton's omission to obtain the solar spectrum under the simplest conditions which prevented him from discovering the dark lines. Using a broad beam of light which had passed through a round hole or a triangular slit, he obtained a brilliant spectrum, but one in which many different coloured rays overlapped each other. In the recent history of the science of the spectrum, one main difficulty has consisted in the mixture of the lines of several different substances, which are usually to be found in the light of any flame or spark. It is seldom possible to obtain the light of any element in a perfectly simple manner. Angström greatly advanced this branch of science by examining the light of the electric spark when formed between poles of various metals, and in the presence of various gases. By varying the pole alone, or the gaseous medium alone, he was able to discriminate correctly between the lines due to the metal and those due to the surrounding gas[k].

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[h] 'Experimental Researches in Electricity,' vol. iii. p. 4.

[i] 'Life of Faraday,' vol. ii. p. 24.

[k] 'Philosophical Magazine,' 4th Series, vol. ix. p. 327.

Failure in the Simplification of Experiments.

In some cases it seems to be impossible to carry out the rule of varying one circumstance at a time. When we attempt to obtain two instances or two forms of experiment in which a single circumstance shall be present or absent, it may be found that this single circumstance entails one or more others. Benjamin Franklin's experiment concerning the comparative absorbing powers of different colours is well known. 'I took,' he says, 'a number of little square pieces of broadcloth from a tailor's pattern card, of various colours. They were black, deep blue, lighter blue, green, purple, red, yellow, white, and other colours and shades of colour. I laid them all out upon the snow on a bright sunshiny morning. In a few hours, the black being most warmed by the sun, was sunk so low as to be below the stroke of the sun's rays; the dark blue was almost as low; the lighter blue not quite so much as the dark; the other colours less as they were lighter. The white remained on the surface of the snow, not having entered it at all.' This is a very elegant and apparently simple experiment; but when Leslie had completed his series of researches upon the nature of heat, he came to the conclusion that the colour of a surface has very little effect upon the radiating power, the mechanical nature of the surface appearing to be more influential. He remarks[1] that 'the question is incapable of being positively resolved, since no substance can be made to assume different colours without at the same time changing its internal structure.' More recent investigation has shown that the subject is one of considerable complication, because the absorptive power of a surface may be different according to the character of the rays which fall upon it;

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[1] 'Inquiry into the Nature of Heat,' p. 95.

but there can be no doubt as to the acuteness with which Leslie points out the difficulty. In Well's investigations concerning the nature of dew, we have, again, very complicated conditions. If we expose plates of various material, such as rough iron, glass, polished metal, to the midnight sky, they will be dewed in various degrees; but since these plates differ both in the nature of the surface and the conducting power of the material, it would not be plain whether one or both circumstances were of importance. We avoid this difficulty by exposing the same material polished or varnished, so as to present different conditions of surface[m]; and again by exposing different substances with the same kind of surface.

When we are quite unable to isolate circumstances we must resort to the procedure described by Mr. J. S. Mill under the name of the Joint Method of Agreement and Difference. We must collect as many instances as possible in which a given circumstance produces a given result, and as many as possible in which the absence of the circumstance is followed by the absence of the result. To adduce his example, we cannot experiment upon the cause of double refraction in Iceland spar, because we cannot alter its crystalline condition without altering it altogether, nor can we find substances exactly like calc spar in every circumstance except one. We can only resort therefore to the method of comparing together all known substances which have the property of doubly-refracting light, and we find that they agree in being crystalline[n]. This indeed is nothing but an ordinary process of perfect or probable induction, already partially described, and to be further discussed under the subject of Classification. It may be added, however, that the subject does admit of perfect experimental treatment, since glass, when strongly compressed, and so long only as it is compressed in one direction, becomes capable of doubly-refracting light, and as there is probably no alteration in the glass but change of elasticity, we learn that the power of double refraction is very probably due to a difference of elasticity in different directions.

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[m] Herschel, 'Preliminary Discourse on the Study of Natural Philosophy,' p. 161.

[n] 'System of Logic,' bk. III. chap. viii. § 4, 5th. ed. vol. i. p. 433.

Removal of Usual Conditions.

One of the great objects of experiment is to enable us to judge of the behaviour of substances under conditions widely different from those which prevail upon the surface of the earth. We live in an atmosphere which does not vary beyond certain narrow limits in temperature or pressure. Many of the powers of nature, such as gravity, which constantly act upon us, are of almost fixed amount. Now it will afterwards be shown that we cannot apply a quantitative law to circumstances much different from those in which it was observed, without considerable risk of error. In the other planets, the sun, the stars, or remote parts of the Universe, the conditions of existence must often be widely different from what we commonly experience here. Hence our knowledge of nature must remain very restricted and hypothetical, unless we can subject substances to very unusual conditions by suitable experiments.

The electric arc is an invaluable means of exposing metals or other conducting substances to the highest known temperature. By its aid we learn not only that all the metals can be vaporized, but that they all give off distinctive rays of light. At the other extremity of the scale, the intensely powerful freezing mixture devised by Faraday, consisting of solid carbonic acid and ether mixed in vacuo, enables us to observe the nature of substances at temperatures immensely below any we meet with naturally on the earth's surface.

We can hardly realize now the importance of the invention of the air-pump, previous to which it was exceedingly difficult to make any experiment except under the ordinary pressure of the atmosphere. The Torricellian vacuum had been employed by the philosophers of the Accademia del Cimento to show the behaviour of water, smoke, sound, magnets, electric substances, &c., in vacuo, but their experiments were often unsuccessful from the difficulty of excluding air[o].

Among the most constant circumstances under which we live is the force of gravity, which does not vary, except by a slight fraction of its amount, in any part of the earth's crust or atmosphere to which we can attain. Now this force is often sufficient to overbear and disguise various actions; for instance, the mutual gravitation of small bodies. It was an interesting experiment of Plateau to withdraw substances from the action of gravity by suspending them in liquids of exactly the same specific gravity. Thus a quantity of oil poured into the middle of a suitable mixture of alcohol and water, assumes a spherical shape which, on being made to rotate, becomes spheroidal, and then successively separates into a ring and a group of spherules. Thus we have at least an illustration of the mode in which the planetary system may have been produced[p], though it is to be remembered that the extreme difference of scale prevents our arguing with confidence from the experiment to the conditions of the nebular theory.

It is possible that the so-called elements are elementary only to us, because we are restricted to temperatures at which they are fixed. Lavoisier carefully defined an element as a substance which cannot be decomposed *by*

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[o] 'Essayes of Natural Experiments made in the Accademia del Cimento.' *Englished* by Richard Waller, 1684, p. 40, &c.

[p] *Plateau,* Taylor's 'Scientific Memoirs,' vol. iv. pp. 16–43.

any known means; but it seems almost certain that some series of elements, for instance Iodine, Bromine, and Chlorine, are really compounds of a simpler substance. We must doubtless look to the production of intensely high temperatures, as yet quite beyond our means, for the decomposition of these so-called elements. But it may very possibly be found that, in this age and part of the universe, the dissipation of energy has so far proceeded that there are no sources of heat left to us sufficiently intense to effect the decomposition of the supposed elements.

Interference of Unsuspected Conditions.

It may often happen that we are not aware of all the conditions under which our researches are made. Some substance may be present or some power may be in action, which escapes the most vigilant examination. Not being aware of its existence, we are of course unable to take proper measures to exclude it, and thus determine the share which it may have in the results of our experiments. There can be little doubt that the alchemists were often misled and encouraged in their vain attempts by the unsuspected presence of traces of gold and silver in the substances they proposed to transmute. Lead, as drawn from the smelting furnace, almost always contains some silver, and gold is associated with many other metals. Thus small quantities of noble metal would often appear as the result of experiment and raise delusive hopes.

In more than one case the unsuspected presence of common salt in the air has caused great trouble. In the early experiments on electrolysis it was found that, when water was decomposed, an acid and an alkali were produced at the poles, together with oxygen and hydrogen. In the absence of any other explanation for this singular result, some chemists rushed to the conclusion that electricity must have the power of *generating* acids and alkalis, and one chemist thought he had discovered a new substance called *electric acid*. But Davy proceeded to a systematic investigation of the circumstances, by varying the conditions. Changing the glass vessel for one of agate or gold, he found that far less alkali was produced; excluding impurities by the use of very carefully distilled water, he found that the quantities of acid and alkali were still further diminished; and having thus obtained a clue to the cause he completed the exclusion of impurities by avoiding contact with his fingers, and by placing the apparatus under an exhausted receiver, no acid or alkali being then detected. It would be difficult to meet with a more elegant or successful case of the detection of a condition previously unsuspected[q].

It is highly remarkable that the presence of common salt in the air, proved to exist by Davy, nevertheless continued a stumbling-block in the science of spectrum analysis, and probably prevented men, such as Brewster, Herschel, and Talbot, from anticipating by thirty years the discoveries of Bunsen and Kirchhoff. As I have elsewhere pointed out[r], the utility of the spectrum was known in the middle of the last century to Thomas Melvill, a talented Scotch physicist, who died at the early age of 27 years[s]. But Melvill was struck in his examination of various coloured flames by the extraordinary predominance of homogeneous yellow light, which was due to some circumstance escaping his attention. Wollaston and

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[q] 'Philosophical Transactions,' [1826] vol. cxvi. pp. 388, 389. Works of Sir Humphry Davy, vol. v. pp. 1–12.

[r] 'National Review,' July, 1861, p. 13.

[s] His published works are contained in 'The Edinburgh Physical and Literary Essays,' vol. ii. p. 34; 'Philosophical Transactions,' [1753] vol. xlviii. p. 261; see also Morgan's Paper in 'Philosophical Transactions,' [1785] vol. lxxv. p. 190.

Fraunhofer were equally struck by the prominence of the yellow line in the spectrum of nearly every kind of light. Talbot expressly recommended the use of the prism for detecting the presence of substances by what we now call spectrum analysis, but he found that all substances, however different the light they yielded in other respects, were identical as regards the production of yellow light. Talbot knew that the salts of soda all gave this coloured light, but in spite of Davy's previous difficulties with salt in electrolysis, it did not occur to him to assert that where the light is, there the sodium must be. He suggested water as the most likely source of the yellow light, because of its usual presence; but even substances which were apparently devoid of water gave the very same yellow light[t]. Brewster and Herschel both experimented upon flames almost at the same time as Talbot, and Herschel unequivocally enounced the principle of spectrum analysis[u]. Nevertheless Brewster, after numerous experiments attended with great trouble and disappointment, found that yellow light might be obtained from the combustion of almost any substance. It was not until 1856 that Professor W. Swan discovered that an almost infinitesimal quantity of sodium chloride, say a millionth part of a grain, was sufficient to tinge a flame of a bright yellow colour. The universal diffusion of the salts of sodium, joined to this unique light-producing power, was thus shown to be the unsuspected circumstance which had destroyed the confidence of all previous experimenters in the use of the prism. Some references concerning the history of this curious point are given below[x].

[t] 'Edinburgh Journal of Science,' vol v. p. 79.

[u] 'Encyclopædia Metropolitana,' article *Light*, § 524; Herschel's 'Familiar Lectures,' p. 266.

[x] Talbot, 'Philosophical Magazine,' 3rd Series, vol. ix. p
Brewster, 'Transactions of the Royal Society of Edinburg

In the science of radiant heat, early inquirers were led to the conclusion that radiation proceeded only from the surface of a solid, or from a very small depth below it. But they happened to experiment upon surfaces covered by coats of varnish, which is highly athermanous or opaque to heat. Had they properly varied the character of the surface, using a highly diathermanous substance like rock salt, they would have obtained very different results[y].

One of the most extraordinary instances of an erroneous opinion due to overlooking interfering agents is that concerning the increase of rainfall near to the earth's surface. More than a century ago it was observed that rainguages placed upon church steeples, house tops, and other elevated places, gave considerably less rain than if they were on the ground, and it has very recently been shown that the variation is most rapid in the close neighbourhood of the ground[z]. All kinds of theories have been started to explain this phenomenon; but I have attempted to show[a] that it is simply due to the interference of wind, which deflects more or less rain from all the guages which are at all exposed to it.

The great magnetic power of iron renders it a constant source of disturbance in all magnetic experiments. In building a magnetic observatory great care must therefore be taken that no iron is employed in the construction, and that no masses of iron are near at hand. In some cases magnetic observations have been seriously disturbed by the existence of masses of iron ore in the neighbourhood. In Faraday's experiments upon feebly magnetic or diamag-

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ix. pp. 433, 455; Swan, ibid. [1856] vol. xxi. p. 411.; 'Philosophical Magazine,' 4th Series, vol. xx. p. 173, [Sept. 1860]; Roscoe, 'Spectrum Analysis,' Lecture III.

[y] Stewart, 'Elementary Treatise on Heat,' p. 192.

[z] British Association, Liverpool, 1870. 'Report on Rainfall,' p. 176.

[a] 'Philosophical Magazine,' Dec. 1861, 4th Series, vol. xxii. p. 421.

netic substances he took the greatest precautions against the presence of any disturbing substance in the copper wire, wax, paper, and other articles used in suspending the test objects. It was his invariable custom to try the effect of the magnet upon the apparatus in the absence of the object of experiment, and without this preliminary trial no confidence could be placed in the results[b]. Tyndall has also employed the same mode for testing the freedom of electro-magnetic coils from iron, and was thus enabled to obtain them devoid of any cause of disturbance[c]. It is well worthy of notice that in the very infancy of the science of magnetism, the acute experimentalist Gilbert correctly accounted for the opinion existing in his day that magnets would attract silver, by pointing out that the silver contained iron[d].

Even when we are not aware by previous experience of the probable presence of a special disturbing agent, we ought not to assume the absence of unsuspected interference. If, then, an experiment is of really high importance, so that any considerable branch of science rests upon it, we ought to try it again and again, in as varied conditions as possible. We should intentionally disturb the apparatus in various ways, so as if possible to hit by accident upon any peculiar weak points. Especially when our results are more regular and accordant than we have fair grounds for anticipating, ought we to suspect some peculiarity in the apparatus which causes it to measure some other phenomenon than that in question, just as Foucault's pendulum almost invariably indicates the revolution of the axes of its own elliptic path instead of the revolution of the globe.

It was in this cautious spirit that Baily acted in his splendid experiments on the density of the earth. The accuracy of his results entirely depended upon the elimination of all disturbing influences, so that the oscillation of his torsion balance should depend on gravity alone. Hence he varied the apparatus in many ways, changing the small balls subject to attraction, changing the connecting rod, and the means of suspension. He observed the effect of artificial disturbances, such as the presence of visitors, the occurrence of violent storms, &c., and as no real alteration was produced in the results, he confidently attributed them to gravity[e].

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[b] 'Experimental Researches in Electricity,' vol. iii. p. 84, &c.

[c] 'Lectures on Heat,' p. 21.

[d] Gilbert, 'De Magnete.'

Newton would probably have discovered the mode of constructing achromatic lenses, but for the unsuspected effect of some sugar of lead which he is supposed to have dissolved in the water of a prism. He tried, by means of a glass prism combined with a water prism, to produce dispersion of light without refraction, and if he had succeeded there would have been an obvious mode of producing refraction without dispersion. His failure is supposed to be due to his adding lead acetate to the water for the purpose of increasing its refractive power, the lead having a high dispersive power which frustrated his purpose[f]. Judging from Newton's remarks, in the 'Philosophical Transactions,' it would appear as if he had not, without many unsuccessful trials, despaired of the construction of achromatic glasses[g].

The Academicians of Cimento, in their early and ingenious experiments upon the vacuum, were often misled by the mechanical imperfections of their apparatus. They concluded that the air had nothing to do with the production of sounds, evidently because their vacuum was not sufficiently perfect[h]. Otto von Guericke fell into a like mistake in the use of his newly-constructed air-pump, doubtless from the unsuspected presence of air sufficiently dense to convey the sound of the bell[i].

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[e] Baily, 'Memoirs of the Royal Astronomical Society,' vol. xiv. pp. 29–30.

[f] Grant's 'History of Physical Astronomy,' p. 531.

[g] 'Philosophical Transactions,' abridged by Lowthorp, 4th edition, vol. i. p. 202.

It is hardly requisite to point out that the doctrine of spontaneous generation is due to the unsuspected presence of germs, even after the most careful efforts to exclude them[k], and in the case of many diseases, both of animals and plants, germs which we have no means as yet of detecting and examining, are doubtless the active cause. It has long been a subject of dispute, again, whether the plants which spring up from newly turned land grow from seeds long buried in that land, or from seeds brought by the wind. Argument is unphilosophical when direct trial can readily be applied; for by turning up some old ground, and covering a portion of it with a glass case, the conveyance of seeds by the wind can be entirely prevented, and if the same plants appear within and without the case, it will become clear that the seeds are in the earth. By gross oversight some experimenters have thought before now that crops of rye had sprung up where oats had been sown[l].

Blind or Test Experiments.

Every correct and conclusive experiment necessarily consists in the comparison of results between two different combinations of circumstances. To give a fair probability that A is the cause of X, I must maintain invariable all surrounding objects and conditions, and I must then show that where A is X is, and where A is not X is not. Now this cannot really be accomplished in a single trial. If, for instance, a chemist places a certain suspected substance in the Marsh's test apparatus, and finds that it gives a small deposit of metallic arsenic, he cannot be sure that the arsenic really proceeded from the suspected substance; for the impurity of the zinc or sulphuric acid might have been the cause of its appearance. It is therefore the practice of chemists to make what they call a *blind experiment*, that is to try whether arsenic appears in the absence of the suspected substance. The same precaution ought to be taken in all important analytical operations. Indeed, it is not merely a precaution, it is an essential part of any experiment. If the blind trial be not made, the chemist merely assumes that he knows what would happen. Whenever we assert that because A and X are found together A is the cause of X, we imply and assume that if A were absent X would be absent. But wherever it is possible, we ought clearly not to leave this as a mere assumption, or even as a matter of inference. Experience is ultimately the basis of all our inferences, but if we can with care bring immediate experience to bear upon the point in question we should not trust to anything more remote and liable to error. When Faraday examined the magnetic properties of the bearing apparatus, in the absence of the substance to be experimented on, he really made a blind experiment (see vol. ii. p. 41).

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[h] 'Essayes of Natural Experiments,' &c. Englished by Richard Waller, p. 50.

[i] Whewell, 'History of the Inductive Sciences,' 3rd edition, vol. ii. p. 246.

[k] Berkeley's 'Introduction to Cryptogamic Botany,' pp. 258, 259.

[l] Dr. Weissenborn, in the new series of 'Magazine of Natural History,' vol. i. p. 574, quoted in 'Vestiges of Creation,' 2nd edition, p. 222.

We ought also, whenever we can, to test the sufficiency and accuracy of any method of experiment by introducing known amounts of the substance or force to be detected. Thus a new analytical process for the quantitative estimation of an element should be tested by performing it upon a mixture compounded so as to contain a known quantity of that element. The accuracy of the gold assay process greatly depends upon the precaution of assaying alloys of gold of exactly known composition[m]. Gabriel Plattes' works give evidence of much scientific spirit, and when discussing the supposed merits of the divining rod for the discovery of subterranean treasure, he sensibly suggests that the rod should be tried in places where veins of metal are known to exist, and, we might add, known not to exist[n].

Negative Results of Experiment.

When we pay proper regard to the imperfection of all measuring instruments and the possible minuteness of effects, we shall see much reason for interpreting with caution the negative results of experiments. We may fail to discover the existence of an expected effect, not because that effect is really non-existent, but because it is of an amount inappreciable to our senses, or confounded with other effects of much greater amount. As in fact there is no limit on *à priori* grounds to the smallness of a phenomenon, we can never, on the grounds of a single experiment, prove the non-existence of a supposed effect. We are always at liberty to assume that a certain amount of effect might have been detected by greater delicacy of measurement. We cannot safely affirm that the moon has no atmosphere at all. We may doubtless show that the atmosphere, if present, is less dense than the air in the so-called vacuum of an air-pump, as did Du Sejour. It is equally impossible to prove that gravity occupies *no time* in transmission. Laplace indeed ascertained that the velocity of propagation of the influence was at least fifty million times greater than that of light[o]; but it does not really follow that it is instantaneous; and were there any means of detecting the action of one star upon another exceedingly distant star, we might possibly find an appreciable interval occupied in the transmission of the gravitating impulse. Newton could not demonstrate the absence of all resistance to matter moving through space, or the adamantine basis of light; but he ascertained by one of the most beautiful experiments with the pendulum, elsewhere more fully described (vol. ii. p. 55), that if such resistance existed, it was in amount less than one five-thousandth part of the external resistance of the air[p].

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[m] Watts, 'Dictionary of Chemistry,' vol. ii. pp. 936, 937.

[n] 'Discovery of Subterraneal Treasure,' London, 1639, p. 48.

Innumerable incidents in the history of science tend to show that phenomena, which one generation has failed to detect, may become accurately known to a succeeding generation. The compressibility of water which the Academicians of Florence could not prove, because at a low pressure the effect was too small to perceive, and at a high pressure the water oozed through their silver vessel[q], has now become the subject of exact measurements and precise calculation. Independently of Newton, Hooke entertained very remarkable notions concerning the nature of gravitation. In this and other subjects he showed, indeed, a genius for experimental investigation which would have placed him in the first rank in any other age than that of Newton. He correctly conceived that the force of gravity would decrease as we receded from the centre of the earth, and he boldly attempted to prove it by experiment. Having exactly counterpoised two weights in the scales of a balance, or rather one weight against another weight and a long piece of fine cord, he removed his

[o] Laplace, 'System of the World,' transl. by Harte, vol. ii. p. 322.

[p] 'Principia,' bk. II. sec. 6, Prop. xxxi. Motte's translation, vol. ii. p. 108.

[r] 'Essayes of Natural Experiments,' &c. p. 117.

balance to the top of the Dome of St. Paul's, and tried whether the balance remained in equilibrium after one weight was allowed to hang down to a depth of 240 feet. No difference could be perceived when the weights were at the same and at different levels, but Hooke rightly held that the failure arose from the insufficient difference of height. He says, 'Yet I am apt to think some difference might be discovered in greater heights[r].' The radius of the earth being about 20,922,000 feet, we can now readily calculate from the known law of gravity that a height of 240 would not make a greater difference than one part in 40,000 of the weight. Such a difference would doubtless be inappreciable in the balances of that day, though it could readily be detected by balances now frequently constructed. Again, the mutual gravitation of bodies at the earth's surface is so small that Newton appears to have made no attempts to demonstrate its existence experimentally, merely remarking that it was too small to fall under the observation of our senses[s]. It has since been successfully detected and measured by Cavendish, Baily and others.

The smallness of the quantities which we can now observe is often very astonishing. A balance will weigh to one millionth part of the load or less. Sir Joseph Whitworth can measure to the one millionth part of an inch. A rise of temperature of the 8800th part of a degree centigrade has been detected by Dr. Joule. The spectroscope can reveal the presence of the one 180,000,000th part of a grain of soda, and the sense of smell can probably feel the presence of a far less quantity of odorous matter[t]. We must nevertheless remember that effects of indefinitely less amount than these must exist, and we should state our negative result with corresponding caution. We can only disprove the existence of a quantitative phenomenon by showing deductively, from the laws of nature, that if present it would amount to a perceptible quantity. As in the case of other negative arguments (vol. ii. p. 19) we must demonstrate that the effect would appear, where it is by experiment found not to appear.

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[r] Hooke's 'Posthumous Works,' p. 182.

[s] 'Principia,' bk. III, Prop. vii. Corollary 1.

[t] Keill's 'Introduction to Natural Philosophy.' 3rd ed., London, 1733. pp. 48–54.

Limits of Experiment.

It will be obvious that there are many operations of nature which we are quite incapable of imitating in our experiments. Our object is to study the conditions under which a certain effect is produced; but one of those conditions may involve a great length of time. There are instances on record of experiments extending over five or ten years, and even over a large part of a lifetime; but such intervals of time are almost nothing to the time during which nature may have been at work. The contents of a mineral vein in Cornwall may have been undergoing gradual change for a million years or more. All metamorphic rocks have doubtless endured high temperature and enormous pressure for almost inconceivable periods of time, so that chemical geology is generally beyond the scope of experiment.

Arguments have been continually brought against Darwin's theory, founded upon the absence of any clear instance of the production of a new species. During an historical period of perhaps four thousand years, no animal, it is said, has been so much domesticated as to become different in species. It might as well be argued, as it seems to me, that no geological changes are taking place, because no new mountain has risen in Great Britain within the memory of man. Our actual experience of geological changes is like a mere point in the infinite progression of time. When we know that rain water falling on limestone will carry away a minute portion of the rock in solution, we do not hesitate to multiply that quantity by millions and millions, and assert that in course of time a mountain may be dissolved away. We have actual experience concerning the rise of land in some parts of the globe and its fall in others to the extent of some feet. Do we hesitate to infer what may thus be done in course of geological ages? As Gabriel Plattes long ago remarked, 'The sea never resting, but perpetually winning land in one place and losing in another, doth shew what may be done in length of time by a continual operation, not subject unto ceasing or intermission'[u]. The action of physical circumstances upon the forms and characters of animals by natural selection is subject to exactly the same remarks. As regards animals living in a state of nature the change of circumstances which can be ascertained to have occurred is so indefinitely slight, that we could not expect to observe any change in those animals whatever. Nature has made no experiment at all for us within historical times. Man, however, by taming and domesticating dogs, cats, horses, oxen, &c., has made considerable change in their circumstances, and we find considerable change also in their forms and character. Supposing the state of domestication to continue unchanged, these new forms would continue permanent so far as we know, and in this sense they are permanent. Thus the arguments against Darwin's theory, founded on the non-observation of natural changes within the historical period, are of the weakest character, being purely negative.

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[u] 'Discovery of Subterraneal Treasure,' 1639, p. 52.

CHAPTER XX.

METHOD OF VARIATIONS.

EXPERIMENTS may be of two kinds: experiments of simple fact, and experiments of quantity. In the first class of experiments we combine certain conditions, and wish to ascertain whether or not a certain effect of any quantity exists. Thus Hooke, as before described, wished to ascertain whether or not there was any difference in the force of gravity at the top and bottom of St. Paul's Cathedral. The chemist continually performs analyses for the purpose of ascertaining whether or not a given element exists in a particular mineral or mixture; all such experiments and analyses are qualitative rather than quantitative, because though the result may be more or less, and is necessarily quantitative, the particular amount of the result is not the immediate object of the enquiry.

So soon, however, as a result is known to be discoverable, the scientific man ought to proceed to the strictly quantitative enquiry, how great a result follows from a certain amount of the conditions which are supposed to constitute the aggregate cause? The possible numbers of experiments are now indefinitely great, for every variation in a necessary condition will usually produce a variation in the amount of the effect. The method of variation which thus arises is no narrow or special method, but it is the general application of experiment to phenomena capable of continuous quantity. As Professor

Fowler has well remarked[a], the observation of variations is really an integration of a supposed infinite number of applications of the so-called method of difference, that is of experiment in its perfect form.

In induction we aim at establishing a general law, and if we deal with quantities that law must really be expressed more or less obviously in the form of an equation, or it may be in more than one equation. We treat as before of conditions, and of what happens under those conditions. But the conditions will now vary, not in quality, but quantity, and the effect will also vary in quantity, so that the result of quantitative induction is always to arrive at some mathematical expression involving the quantity of each condition, and expressing the quantity of the result. In other words, we wish to know what function the effect is of its conditions. We shall find that it is one thing to obtain the numerical results, and quite another thing to detect the law obeyed by those results, the latter being an operation of an inverse and tentative character.

The Variable and the Variant.

Almost every series of quantitative experiments is directed to obtain the relation between the different values of one quantity which is varied at will, and another quantity which is caused thereby to vary. We may conveniently distinguish these as respectively the *variable* and the *variant.* When we are examining the effect of heat in expanding bodies, heat, or one of its dimensions, temperature, is the variable, length the variant. If we compress a body to observe how much it is thereby heated, pressure, or it may be the dimensions of the body, forms the variable, heat the variant. In thermo-electric pile we make heat the variable and the measure electricity as the variant. That one of the two measured quantities which is an antecedent condition of the other will be the variable.

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[a] 'Elements of Inductive Logic,' 1st edit. p. 175.

It will always be convenient to have the variable entirely under our command. Experiments may indeed be made with accuracy, provided we can exactly measure the variable at the moment when the quantity of the effect is determined by it. But if we have to trust to the action of some capricious and very uncertain force, there may be great difficulty in making exact measurements, and those results may not be disposed over the whole range of quantity in a convenient manner. It is one prime object of the experimenter, therefore, to obtain a regular and governable supply of the cause or force which he is investigating. To determine correctly the efficiency of windmills, when the natural winds were constantly varying in force, would be exceedingly difficult. Smeaton, therefore, in his experiments on the subject, created a uniform artificial wind of the required force by moving his models against the air on the extremity of a revolving arm[b]. The velocity of the wind could thus be rendered greater or less, it could be maintained uniform for any length of time, and its amount could be exactly ascertained. In determining the laws of the chemical action of light it would be out of the question to employ the rays of the sun, which vary in intensity with the clearness of the atmosphere, and with every passing cloud. One great source of difficulty in photometry and the experimental investigation of the chemical action of light consists in obtaining a perfectly uniform and governable source of light rays[c].

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[b] 'Philosophical Transactions,' vol. li. p. 138; abridgment, vol. xi. p. 355.

[c] See Bunsen and Roscoe's 'Researches,' in 'Philosophical Transactions' (1859), vol. cxlix. p. 880, &c., where they describe a constant flame of carbon monoxide gas.

Fizeau's method of measuring the velocity of light enabled him to appreciate the time occupied by light in travelling through a distance of eight or nine thousand metres. But the revolving mirror of Wheatstone subsequently enabled Foucault and Fizeau to measure the velocity in a space of four metres. In this latter method there was the obvious advantage that various media could be substituted for air, and the temperature, density, and other conditions of the experiment accurately governed or defined.

Measurement of the Variable.

There is little use in obtaining exact measurements of an effect unless we can also exactly measure the conditions with which the effect is to be connected. It is absurd to measure the electrical resistance of a piece of metal, its elasticity, tenacity, density, or other physical qualities, if these vary in degree, not only with the minute and almost inappreciable impurities of the metal, but also with its physical condition. If the same bar changes its properties by being heated and cooled, and we cannot exactly define the state in which it is at any moment, our care in measuring will be wasted, because it can lead to no law. It is of little use to determine very exactly the electric conductibility of carbon, which as graphite or gas carbon conducts like a metal, as diamond is almost a non-conductor, and in several other forms possesses variable and intermediate powers of conduction. It will be of use only for immediate practical applications. Before measuring these we ought to have something to measure of which the conditions are capable of exact definition, and to which at a future time we or others can recur. Similarly the accuracy of our measurement need not much surpass the accuracy with which we can define the conditions of the object treated.

The speed of electricity in passing through a conductor mainly depends upon the inductive capacity of the surrounding substances, and, except for technical or special purposes, there is little use in measuring velocities which in some cases are one hundred times as great as in other cases. But the maximum speed of electric conduction is probably a constant quantity of great scientific importance, and according to Prof. Clerk Maxwell's determination in 1868 is 174,800 miles per second, or little less than that of light. The true boiling point of water is a point on which all practical thermometry depends, and it is highly important to determine that point in relation to the absolute thermometric scale. But when water free from air and impurity is heated there seems to be no definite limit to the temperature it may reach, a temperature of 356° Fahr. having been actually observed. Such temperatures, therefore, do not require very accurate measurement. All meteorological measurements depending on the accidental condition of the sky are of infinitely less importance than physical measurements in which such accidental conditions do not intervene. Many profound investigations depend upon our knowledge of the radiant energy continually poured upon the earth by the sun; but this must be measured when the sky is perfectly clear, and the absorption of the atmosphere at its minimum. The slightest interference of cloud destroys the value of such a measurement, except for meteorological purposes, which are of vastly less generality and importance. It is seldom useful, again, to measure such a quantity as the height of a snow-covered mountain within a foot, when the thickness of the snow alone may cause it to vary 25 feet or more, when in short the height itself is indefinite to that extent[c].

[c] Humboldt's 'Cosmos' (Bohn), vol. i. p. 7.

Maintenance of Similar Conditions.

Our ultimate object in induction must be to obtain the complete relation between the conditions and the effect, but this relation will generally be so complex that we can only attack it in detail. We must, as far as possible, confine the variation to one condition at a time, and establish a separate relation between each condition and the effect. This will be at any rate the first step in approximating to the complete law, and it will be a subsequent question how far the simultaneous variation of several conditions modifies their separate actions. In many of the most important experiments, indeed, it is only one condition which we wish to study, and the others are merely interfering forces which we would gladly avoid if possible. One of the conditions of the motion of a pendulum is the resistance of the air, or other medium in which it swings; but when Newton was desirous of proving the equal gravitation of all substances, he had no interest in so entirely different a force as the effect of the air. His object was then to observe a single force only, and so it is in a great many other experiments. Accordingly one of the most important methods of investigation consists in maintaining all the conditions of like magnitude except that which is to be studied. As that admirable experimental philosopher, Gilbert, expressed it[f], 'There is always need of similar preparation, of similar figure, and of equal magnitude, for in dissimilar and unequal circumstances the experiment is doubtful.'

In Newton's decisive experiment similar conditions were provided for, with the usual simplicity which characterizes the highest art. The pendulums of which the oscillations were compared consisted of exactly equal boxes of wood, hanging by equal threads, and filled with different substances, so that the total weights should be exactly equal and the centres of oscillation at the same distance from the points of suspension. Hence the resistance of the air became approximately a matter of indifference; for the outward size and shape of the pendulums being exactly the same, the absolute force of resistance would be the same, so long as the pendulums vibrated with equal velocity; and the weights being equal the force would diminish the velocity in like degree. Hence if any inequality were observed in the vibrations of the two pendulums, it must arise from the only circumstance which was different, namely the chemical character of the matter within the boxes. No inequality being observed, the chemical nature of substances can have no appreciable influence upon the force of gravitation[g].

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[f] Gilbert, 'De Magnete,' p. 109

A beautiful experiment was devised by Dr. Joule for the purpose of showing that the gain or loss of heat by a gas is connected, not with the mere change of its volume and density, but with the energy received or given out by the gas. Two strong vessels, connected by a tube and stop-cock, were surrounded entirely with water after the air had been exhausted from one vessel and condensed in the other to the extent of twenty atmospheres. The whole apparatus having been brought to a uniform temperature by agitating the water, and the temperature having been exactly observed, the stop-cock was opened, so that the air at once expanded and filled the two vessels uniformly. The temperature of the water being again noted was found to be almost entirely unchanged. The experiment was then repeated in an exactly similar manner, except that the strong vessels were placed in separate portions of water. It was then discovered that cold was produced in the vessel from which the air rushed, and an almost exactly equal quantity of heat appeared in that to which it was conducted. Thus Dr. Joule clearly proved th rarefaction produces as much heat as cold, and that onl when there is a disappearance of mechanical energy wil there be production of heat[h]. What we have to notice, however, is not so much the result of the experiment, as the admirably simple manner in which a single change in the apparatus, the separation of the portions of water surrounding the strong air vessels, is made to give indications of the utmost significance.

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[g] 'Principia,' bk. III. Prop. vi.

Collective Experiments.

There is an interesting class of experiments which enable us to observe an indefinite number of quantitative results in one act. Generally speaking, each experiment yields us but one number, and before we can approach the real processes of reasoning we must laboriously repeat measurement after measurement, until we can lay out a pretty complete curve of the variation of one quantity as depending on another. Now we can sometimes abbreviate this labour, by making one quantity vary in different parts of the same apparatus through every required mount. Thus in observing the height to which water ises by the capillary attraction of a glass vessel, we may ke a series of glass tubes of different bore, and measure e height through which it rises in each. But if we ke two glass plates, and place them vertically in water, as to be in contact at one vertical side, and slightly arated at the other side, the interval between the tes varies through every intermediate width, and the er rises to a corresponding height, producing at its er surface a hyperbolic curve.

he absorption of light in passing through a coloured d may be beautifully shown by enclosing the liquid in a wedge-shaped glass, so that we have at a single glance an infinite variety of thicknesses in view. As Newton himself remarked, a red liquid viewed in this manner is found to have a pale yellow colour at the thinnest part, and it passes through orange into red, which gradually becomes of a deeper and darker tint[i]. The effect may be noticed even in a common conical wine-glass. The prismatic analysis of light from such a wedge-shaped vessel discloses the reason, by exhibiting the progressive absorption of different rays of the spectrum as investigated by Dr. J. H. Gladstone[k].

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[h] 'Philosophical Magazine,' 3rd Series, vol. xxvi. p. 375.

A moving body may sometimes be made to mark out its own course, like a shooting star which leaves a tail behind it. Thus an inclined jet of water exhibits in the clearest manner the parabolic path of a projectile. In Wheatstone's Kaleidophone the curves produced by the combination of vibrations of different ratios are shown by placing bright reflective buttons on the tops of wires of various forms. The motions are performed so quickly that the eye receives the impression of the path as a complete whole, just as a burning stick whirled round produces a continuous circle. The laws of electric induction are beautifully shown when iron filings are brought under the influence of a magnet, and fall into curves corresponding to what Faraday called the Lines of Magnetic Force. When Faraday tried to define what he meant by his lines of force, he was obliged to refer to the filings. 'By magnetic curves,' he says[l], 'I mean lines of magnetic forces which would be depicted by iron filings.' Robison had previously produced similar curves by the action of frictional electricity[m], and from a mathematical investigation of the forms of such curves we may infer that magnetic and electric attractions obey the general law of emanation, that of the inverse square of the distance. In the electric brush we have another similar exhibition of the laws of electric attraction.

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[i] 'Opticks,' 3rd edit. p. 159.

[k] Watts, 'Dictionary of Chemistry,' vol. iii. p. 637.

[l] 'Faraday's Life,' by Bence Jones, vol. ii. p. 5.

[m] Watts 'Dictionary of Chemistry,' vol. ii. pp. 402, 403.

There are several branches of science in which collective experiments have been used with great advantage. Lichtenberg's electric figures, produced by scattering electrified powder on an electrified resin cake, so as to show the condition of the latter, suggested to Chladni the notion of discovering the state of vibration of plates by strewing sand upon them. The sand collects at the points where the motion is least, and we gain at a glance a comprehension of the general form of undulation of the whole plate. To this method of experiment we owe the beautiful observations of Savart. The exquisite coloured figures exhibited by plates of crystal, when examined by polarized light, afford a more complicated example of the same kind of investigation. They led Brewster and Fresnel to a successful explanation of the properties of the optic axes of crystals. The unequal conduction of heat in crystalline substances has also been shown in a similar manner, by spreading a thin layer of wax over the plate of crystal, and applying heat to a single point. The wax then melts in a circular or elliptic area according as the rate of conduction is uniform or not. Nor should we forget that Newton's rings were an early and most important instance of investigations of the same kind, showing the effects of interference of light undulations of all magnitudes at a single view. Sir John Herschel gave to all such opportunities of observing directly the results of a general law, the name of *Collective Instances*[n], and I propose to adopt the name *Collective Experiments*.

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[n] 'Preliminary Discourse,' &c, p. 185.

Such experiments will in many subjects only give the first hint of the nature of the law in question, but will not admit of any exact measurements. The parabolic form of a jet of water may well have suggested to Galileo his views concerning the path of a projectile; but it would not serve now for the exact investigation of the laws of gravity. It is not likely too that capillary attraction could be exactly measured by the use of inclined plates of glass, and the tubes would probably be better for precise investigation. As a general rule, these collective experiments would be most useful for popular instruction and illustration of the laws of science. But when the curves and figures produced are of a precise and permanent character, as in the coloured figures produced by crystalline plates, they may admit of exact measurement, and may often be the only mode of approaching the question. Newton's rings, diffraction fringes, and other effects of the interference of light, allow of very accurate measurements.

Under the class of collective experiments we may perhaps place those in which we render visible the motions of a mass of gas or liquid by diffusing some opaque substance in it. The behaviour of a body of air may often be studied in a beautiful way by the use of smoke, as in the production of smoke rings and jets. In the case of liquids lycopodium powder is sometimes employed. To detect the mixture of currents or strata of liquid, I employed exceedingly dilute solutions of common salt and silver nitrate, which produce a very visible cloud wherever they come into contact[o]. Atmospheric clouds often reveal to us the movements of great volumes of air which would otherwise be quite unapparent.

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[o] 'Philosophical Magazine,' July, 1857, 4th Series, vol. xiv. p. 24.

Periodic Variations.

A very large and important class of investigations are concerned with Periodic Variations. We may define a periodic phenomenon as one which, with the constant and uniform change of the variable, returns time after time to the same value. If we strike a pendulum it presently returns to the point from which we disturbed it, and with the uniform progress of time goes on making excursions and returning, until stopped by the dissipation of its energy. If one body in space approaches by gravity towards another, they will revolve round each other in an elliptic orbit, and return for an indefinite number of times to the same relative positions. On the other hand a single body projected into empty space, away from the action of any extraneous force, would go on moving for ever in a straight line, according to the first law of motion. In the latter case the variation is called *secular*, because it proceeds during ages in a similar manner, and suffers no περίοδος or going round. It may be doubted whether there really is any motion in the universe which is not periodical. Mr. Herbert Spencer long since adopted the doctrine that all motion is ultimately rhythmical[p], and abundance of evidence may be adduced in favour of his view. The so-called secular acceleration of the moon's motion is certainly periodic, and as, so far as we can tell, no body is beyond the attractive power of other bodies, rectilinear motion becomes purely hypothetical, or at least infinitely improbable. All the motions of all the stars must tend to become periodic. Though certain disturbances in the planetary system seem to be uniformly progressive, Laplace is considered to have proved that they really have their limits, so that after an almost infinitely great time, all the planetary bodies might return to the same exact places, and the stability of the system be established.

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[p] 'First Principles,' 3rd edit. chap. x. p. 253.

But any such theory of periodic stability is really hypothetical, and does not take into account a multitude of phenomena resulting in the dissipation of energy, which may be a really secular process incapable of restoration. For our present purposes we really need not attempt to form any opinion on such lofty questions. Any change which does not present the appearance of a periodic character will be empirically regarded as a secular change for the present, so that there will be an abundant supply of non-periodic variations.

The variations which we produce experimentally will often be non-periodic. When we communicate heat to a gas it increases in bulk or pressure, and as far as we can go the higher the temperature the higher the pressure. Our experiments are of course restricted in temperature both above and below, but there is every reason to believe that the bulk being the same, the pressure would never return to the same point at any two different temperatures. We may of course repeatedly raise and lower the temperature at regular or irregular intervals entirely at our will, and the pressure of the gas will vary in like manner and exactly at the same intervals, but such an arbitrary series of changes would not constitute Periodic Variation. It would constitute a succession of distinct experiments, which would place beyond reasonable doubt the connexion of cause and effect.

Whenever a phenomenon recurs at equal or nearly equal intervals, there is, according to the theory of probability, considerable evidence of connexion, because if the recurrences were entirely casual it is exceedingly unlikely that they would happen at equal intervals. Thus the mere fact that a brilliant comet had appeared in the years 1301, 1378, 1456, 1531, 1607, and 1682, gave considerable presumption in favour of the identity of the body apart from the similarity of the orbit. There is nothing which so strongly fascinates the attention of men as the recurrence time after time of some unusual event. Things and appearances which remain ever the same, ike mountains and valleys, fail to excite the curiosity of a primitive people. It has been remarked by Laplace that even in his day the rising of Venus in its brightest phase never failed to excite surprise and interest. So there is little doubt that the first germ of physical science arose in the attention given by Eastern people to the changes of the moon and the motions of the planets. One of the earliest astronomical discoveries must have consisted in proving the identity of the morning and evening stars, on the ground of their similarity of aspect and invariable alternation[q]. Periodical changes of a somewhat complicated kind must have been understood by the Chaldæans, because they were aware of the cycle of 6585 days or 19 years which brings round the new and full moon upon the same days, hours, and even minutes of the year. The earliest efforts of scientific prophecy were founded upon this knowledge, and if at present we cannot help wondering at the precise anticipations of the nautical almanack, we may readily imagine the wonder excited by such successful predictions in early times.

Combined Periodic Changes.

We shall seldom or never find a body subject to a single periodic variation, and free from any other disturbances. As a general rule we may expect the periodic variation tself to undergo variation, which may possibly be secular or incapable of repetition, but is more likely to prove periodic likewise; nor is there any limit to the complication of periods beyond periods, or periods within periods, which may ultimately be disclosed. In studying, then, a phenomenon of rhythmical character we have a succession of questions to ask. Is the periodic variation uniform? If not, is the change uniform? If not, is the change itself periodic? Is that new period uniform, or subject to any other change, or not? and so on *ad infinitum*.

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[q] Laplace, 'System of the World,' vol. i. pp. 50, 54, &c.

In some cases there may be many distinct causes of periodic variations, and according to the principle of the superposition of small effects, to be afterwards more fully considered, these periodic effects will be simply added together, or at least approximately so, and the joint result may present a very complicated subject of investigation. Thus the tides of the ocean consist of a series of superimposed undulations, of which the number and character have by no means been determined as yet. Not only are there the ordinary and very obvious semi-diurnal tides caused by sun and moon, but a series of minor tides, such as the lunar diurnal, the solar diurnal, the lunar monthly, the lunar fortnightly, the solar annual and solar semi-annual are gradually being disentangled by the labours of Sir W. Thomson and others[r].

Variable stars present very interesting periodic phenomena; while some stars, δ Cephei for instance are subject to very regular and equal variations, others, like Mira Ceti, are less constant in the degrees of brilliancy which they attain or the rapidity of the changes, possibly on account of some much longer periodic variation[s]. The star β Lyræ presents a double maximum and minimum in each of its periods of nearly 13 days, and since the discovery of this variation the period in a period has probably been on the increase. 'At first the variability was more rapid, then it became gradually slower; and this decrease in the length of time reached its limit between the years 1840 and 1844. During that time its period was nearly invariable; at present it is again decidedly on the decrease[t]." It is evident that the tracing out of such complicated variations presents an almost unlimited field for interesting investigation. The number of such variable stars already known is considerable, and there is no reason to suppose that any appreciable fraction of the whole number has yet been detected.

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[r] 'British Association Report,' 1870, p. 120.

[s] Herschel's 'Outlines of Astronomy,' 4th edit. pp. 555–557.

Principle of Forced Vibrations.

All investigations of the connection of periodic causes and effects rest upon a most important and general principle, which has been demonstrated by Sir John Herschel for some special cases, and clearly explained by him in several of his works[u]. The principle may be formally stated in the following manner: 'If one part of any system connected together either by material ties, or by the mutual attractions of its members, be continually maintained by any cause, whether inherent in the constitution of the system or external to it, in a state of regular periodic motion, that motion will be propagated throughout the whole systems, and will give rise, in every member of it, and in every part of each member, to periodic movements executed in equal period, with that to which they owe their origin, though not necessarily synchronous with them in their maxima and minima.' The meaning of the proposition is that the effect of a periodic cause will be periodic, and will recur at intervals equal to those of the cause. Accordingly whenever we find any two phenomena which do proceed, time after time, through changes of exactly the same period, there is much probability that they are connected. It was in this manner, doubtless, that Pliny correctly conjectured that the cause of the tides lay in the sun and moon, the intervals between successive high tides being equal to the intervals between the moon's passage across the meridian. Kepler and Descartes too admitted the connection previous to Newton's demonstration of its precise nature. When Bradley discovered the apparent motion of the stars arising from the aberration of light, he was soon able to attribute it to the earth's annual motion, because it went through all its phases in exactly a year.

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[t] Humboldt's 'Cosmos' (Bohn), vol. iii. p. 229.

[u] 'Encyclopædia Metropolitana,' art. *Sound*, § 323; 'Outlines of Astronomy,' 4th edit. § 650, pp. 410, 487-88; 'Meteorology,' Reprint, p. 137.

The most extensive and beautiful instance of induction concerning periodic changes which can be cited, is that of the discovery of an eleven-year period in various meteorological and astronomical phenomena. It would be difficult to mention any two things apparently more disconnected than the spots upon the sun and auroras. As long ago as 1826, Schwabe, of Dessau, commenced a regular series of observations of the spots upon the sun, which has been continued to the present time, and he was able to show that at intervals of about eleven years the spots increased much in size and number. Hardly was this discovery made known, than Dr. Lamont pointed out a nearly equal period of variation in the magnetic needle as regards declination. The occasional magnetic storms or sudden irregular disturbances of the needle were next shown to take place most frequently at the times when sun spots were prevalent, and as auroras are generally coincident with magnetic storms, these strange phenomena were brought into the cycle[x]. It has since been shown by

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[x] 'Nature,' vol. i. p. 284; Quetelet, 'Sur la Physique du Globe,' pp. 148, 262-64, &c.

Professor Piazzi Smyth and Mr. E. J. Stone, that the temperature of the earth's surface as indicated by sunken thermometers gives some evidence of a like period. The existence of a periodic cause having once been established, it is quite to be expected, according to the principle of forced vibrations, that its influence will be more or less considerable in all meteorological phenomena.

Perhaps the most mysterious part of these investigations is that which refers the phenomena to the planetary configurations as an ulterior cause. Professor Balfour Stewart, with Messrs. Warren de la Rue and Loewy, by laborious researches discovered a periodic change of 584 days in the sun spots, coincident with changes in the relative positions of the Earth, Jupiter, and Venus. It has since been rendered probable by the researches of Dr. Kirkwood and others, that Schwabe's eleven-year period is due to the action of Mercury. Several other periods of more or less importance have been supposed to exist, but the subject is yet open to much more inquiry.

Integrated Variations.

In considering the infinite variety of modes in which one effect may depend upon another, we must set apart in a distinct class those which arise from the accumulated effects of a constantly acting cause. When water runs out of a cistern, the velocity of motion depends, according to Torricelli's theorem, on the height of the surface of the water above the vent; but the amount of water which leaves the cistern in a given time depends upon the aggregate result of that velocity, and is only to be ascertained by the mathematical process of integration. When one gravitating body falls towards another, the force of gravity varies according to the inverse square of the distance; to obtain the velocity produced we must integrate or sum the effects of that law; and to obtain the space passed over by the body in any given time, we must again integrate with regard to the variable velocity.

In periodic variations the same distinction must be drawn. The heating power of the sun's rays at any place on the earth varies every day with the height attained, and is greatest about noon; but it does not follow that the temperature of the air is greatest at the same time. This temperature is an integrated effect of the sun's heating power, and as long as the sun is able to give more heat to the air than the air loses in any other way, the temperature continues to rise, so that the maximum is deferred until about 3 P.M. Similarly the hottest day of the year falls, on an average, about one month later than the summer solstice, and all the seasons lag about a month behind the motions of the sun. In the case of the tides, too, the effect of the sun's or moon's attractive power is never greatest when the power is greatest; the effect always lags more or less behind the cause. Yet the intervals between the successive tides are exactly equal, in the absence of disturbance, to the intervals between the passage of the sun or moon across the meridian. Thus the principle of forced vibrations holds true of all such cases.

In periodic phenomena, however, very curious results will sometimes follow from the integration of effects. If we strike a pendulum, and then repeat the stroke time after time when it is in the same part of the vibration, every stroke concurs with every other one in adding to the momentum, and we can thus increase the extent and violence of the vibrations to any degree. We can stop the pendulum again by strokes applied when it is moving in the opposite direction, and the successive effects being added together will soon bring it to rest. Now if we alter the intervals of the strokes so that each two successive strokes act in opposite manners they will exactly neutralize each other, and the energy expended will be turned into heat or sound at the point of percussion. Exactly similar effects occur in all cases of rhythmical motion. If the musical note C is sounded in a room containing a piano, the string corresponding to it will be thrown into vibration, because every successive stroke of the air-waves upon the string finds it in like position as regards the vibration, and thus adds to its energy of motion. But the other strings being incapable of vibrating with the same rapidity are struck at various periods of their vibrations, and one stroke will sooner or later be opposed by one contrary in effect. All phenomena of *resonance* arise from this coincidence in time of undulation. The air in a pipe closed at one end, and about 12 inches in length, is capable of vibrating 512 times in a second. If, then, the note C is sounded in front of the open end of the pipe, every successive vibration of the air is treasured up as it were in the motion of the air. In a pipe of different length the pulses of air would strike each other, and the mechanical energy would be transmuted into heat and become no longer perceptible as sound.

These accumulated vibrations may sometimes become so intense as to lead to unexpected results. A glass vessel if touched with a violin bow at a suitable point may be fractured with the excess of vibration. In the same way a suspension bridge may readily be broken down if a company of soldiers walk across it in steps the intervals of which happen to agree with the intervals of vibration of the bridge itself. But if they break the step or march with very different time, they may have no perceptible effect upon the bridge. In fact if the impulses communicated to any vibrating body are exactly synchronous with its vibrations, the energy of those vibrations will be unlimited, and may fracture any body.

Let us now consider what will happen if the strokes be not exactly at the same intervals as the vibrations of the body, but, say, a very little slower. Then a succession of strokes will meet the body in nearly but not quite the same position, and their effects will be accumulated. Afterwards the strokes will begin to fall when the body is in the opposite phase. Thus imagine that one pendulum moving exactly from one extreme point to another in a second, should be struck by another pendulum which makes 61 beats in a minute; then, if the pendulums commence together, they will at the end of 30½ beats be moving in opposite directions. Hence whatever energy was communicated in the first half minute will be neutralized by the opposite effect of that given in the second half. The effect of the strokes of the second pendulum will therefore be alternately to increase and decrease the vibrations of the first, so that a new kind of vibration will be produced running through all its phases in 61 seconds. An effect of this kind was actually observed by Ellicott, a member of the Royal Society, in the case of two clocks[y]. He found that through the wood-work by which the clocks were connected a slight impulse was transmitted, and each pendulum alternately lost and gained momentum. Each clock, in fact, tended to stop the other at regular intervals, and in the intermediate times to be stopped by the other. Many of the most important disturbances in the planetary system depend upon the same principle; for if one planet happens always to pull another in the same direction in similar parts of their orbits, the effects, however slight, will be accumulated, and a disturbance of large ultimate amount and of long period will be produced. The long inequality in the motions of Jupiter and Saturn is thus due to the fact that five times the mean motion of Saturn is very nearly equal to twice the mean motion of Jupiter, causing a coincidence in their relative positions and disturbing powers[z].

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[y] 'Philosophical Transactions' (1739), vol. xli. p. 126.

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[z] Grant's 'History of Physical Astronomy,' p. 59.

CHAPTER XXI.

THEORY OF APPROXIMATION.

In order that we may gain a true understanding of the kind, degree, and value of the knowledge which we acquire by experimental investigation, it is requisite that we should be fully conscious of its approximate character. We must learn to distinguish between what we can know and cannot know—between the questions which admit of solution, and those which only seem to be solved. Many persons may be misled by the expression *exact science*, and may think that the knowledge acquired by scientific methods admits of our reaching absolutely true laws, exact to the last degree. There is even a prevailing impression that when once mathematical formulæ have been successfully applied to a branch of science, this portion of knowledge assumes a new nature, and admits of reasoning of a higher character than those sciences which are still unmathematical.

The very satisfactory degree of accuracy attained in the science of astronomy gives a certain plausibility to erroneous notions of this kind. Some persons no doubt consider it to be *proved* that planets move in ellipses, in such a manner that all Kepler's laws hold exactly true; but there is a double error in any such notions. In the first place, Kepler's laws are *not proved*, if by proof we mean certain demonstration of their exact truth. In the next place, even assuming Kepler's laws to be exactly true in a theoretical point of view, the planets never move according to those laws. Even if we could observe the motions of a planet, of a perfect globular form, free from all perturbing or retarding forces, we could never perfectly prove that it moved in an ellipse. To prove the elliptical form we should have to measure infinitely small angles, and infinitely small fractions of a second; we should have to perform impossibilities. All we can do is to show that the motion of an unperturbed planet approaches *very nearly* to the form of an ellipse, and the more nearly the more accurately our observations are made. But if we go on to assert that the path *is* an ellipse we pass beyond our data, and make an assumption which may be more or less probable, but cannot be proved, in the strict sense of that term.

But, secondly, as a matter of fact no planet does move in a perfect ellipse, or manifest the truth of Kepler's laws exactly. The very law of gravity prevents its own results from being clearly exhibited, because the mutual perturbations of the planets distort the elliptical paths. Those laws again hold exactly true only of infinitely small planetary bodies, and when two great globes, like the sun and Jupiter, attract each other, the law must be modified. The periodic time is then shortened in the ratio of the square root of the number expressing the sun's mass, to that of the sum of the numbers expressing the masses of the sun and planet, as was shown by Newton[a]. Even at the present day discrepancies exist between the observed dimensions of the planet's orbits and their theoretical magnitudes, after making allowance for all disturbing causes[b]. Nothing, in fact, is more certain in scientific method than that approximate coincidence can alone be expected. In the measurement of continuous quantity perfect correspondence must be purely accidental, and should give rise to suspicion rather than to satisfaction.

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[a] 'Principia,' bk. III. Prop. 15.

[b] See Lockyer's 'Lessons in Elementary Astronomy,' p. 301.

One remarkable result of the approximate character of our observations is that we never could prove the existence of perfectly circular or parabolic movement, even if it existed. The circle is a singular case of the ellipse, for which the eccentricity is zero; it is infinitely improbable than any planet, even if undisturbed by other bodies, should have a circle for its orbit; but if the orbit were a circle we could never prove the entire absence of eccentricity. All that we could do would be to declare the divergence from the circular form to be inappreciable. Delambre was unable to detect the slightest ellipticity in the orbit of Jupiter's first satellite, but he could only infer that the orbit was *nearly* circular. The parabola is the singular limit between the ellipse and the hyperbola. As there are elliptic and hyperbolic comets, so we might conceive the existence of a parabolic comet. Indeed if an undisturbed comet fell towards the sun from an infinite distance it would move in a parabola; but we could never prove that it so moved.

Substitution of Simple Hypotheses.

In truth men never can solve problems fulfilling the complex circumstances of nature. All laws and explanations are in a certain sense hypothetical, and apply exactly to nothing which we can know to exist. In place of the actual objects which we see and feel, the mathematician invariably substitutes imaginary objects, only partially resembling those represented, but so devised that the discrepancies may not be of an amount to alter seriously the character of the solution. When we probe the matter to the bottom physical astronomy is as hypothetical as Euclid's elements. There may exist in nature perfect straight lines, triangles, circles, and other regular geometrical figures; to our science it is a matter of indifference whether they do or do not exist, because in any case they must be beyond our powers of appreciation. If we submitted a perfect circle to the most rigorous scrutiny and measurement, it is impossible that we should discover whether it were perfect or not. Nevertheless in geometry we argue concerning perfect rectilineal figures and curves, and the conclusions apply to existing objects so far as we can assure ourselves that they agree with the hypothetical conditions of our reasoning. Now this is in reality all that we can do in the most perfect of the sciences of nature.

Doubtless in astronomy we meet with the nearest approximation to actual conditions. The law of gravity is not a complex one in itself, and we believe it with much probability to be exactly true; but we cannot calculate out in any one case its accurate results. The law asserts that every particle of matter in the universe attracts every other particle, with a force depending on the masses of the particles and their distance. We cannot then know the force acting on any one particle unless we know the masses and distances and positions of all the other particles in the universe. The physical astronomer has from the first made a sweeping assumption, namely, that all the other millions of existing systems exert no perturbing effects in our planetary system, that is to say, no effects in the least appreciable. Thus the problem becomes at once hypothetical, because there is little doubt that gravitation between our sun and planets and other systems must exist in some degree. But even when they consider the relations of our planetary bodies *inter se*, all their processes are grossly approximative. In the first place they assume that each of the planets is a perfect ellipsoid, with a smooth surface and a homogeneous interior. That this assumption is untrue every mountain and valley, every are indefinitely more numerous and complex than the few larger terms which are retained. All then is merely approximate.

Concerning other branches of physical science the same general statements are even more evidently true. We speak and calculate about inflexible bars, inextensible lines, heavy points, homogeneous substances, uniform spheres, perfect fluids and gases, and we deduce an infinite number of beautiful theorems; but all is hypothetical. There is no such thing as an inflexible bar, an inextensible line, nor any one of the other perfect objects of mechanical science; they are to be classed with those other almost mythical existences, the straight line, triangle, circle, rectangle, &c., about which Euclid so freely discoursed. Take the simplest operation considered in statics—the use of a crowbar in raising a heavy stone, and we shall find, as Thomson and Tait have pointed out, that we neglect far more than we observe[d]. If we suppose the bar to be quite rigid, the fulcrum and stone perfectly hard, and the points of contact real points, we might give the true relation of the forces. But in reality the bar must bend, and the extension and compression of different parts involve us in difficulties. Even if the bar be homogeneous in all its parts, there is no mathematical theory capable of determining with accuracy all that goes on; if, as is infinitely more probable, the bar is not homogeneous, the complete solution will be indefinitely more complicated, but hardly more hopeless. No sooner had we determined the change of form according to simple mechanical principles, than we should discover the interference of thermodynamic principles. Compression produces heat and extension cold, and thus the conditions of the problem are modified throughout. In attempting a fourth approximation we should have to allow for the conduction of heat from one part of the bar to another. All these effects are utterly inappreciable in a practical point of view, if the bar be a good stout one; but in a theoretical point of view they entirely prevent our saying that we have solved a natural problem. The faculties of the human mind, even when aided by the wonderful powers of abbreviation conferred by analytical methods, are utterly unable to cope with the complications of any one real problem. And had we exhausted all the known phenomena of a mechanical problem, how can we tell that hidden phenomena, as yet undetected, do not intervene in the commonest actions. It is plain that no phenomenon comes within the sphere of our senses unless it possesses a certain momentum or magnitude capable of irritating the appropriate nerves. There may then, and, in fact, must be indefinite worlds of phenomena too slight to rise within the scope of our consciousness.

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[d] 'Treatise on Natural Philosophy,' vol. i. pp. 337, &c.

All the instruments with which we perform our measurements are fallible and faulty. We assume that a plumb-line gives a perfectly vertical line; but this is never true in an absolute sense, owing to the attraction of mountains and other inequalities in the surface of the earth. In an accurate trigonometrical survey, the divergencies of the plumb-line must be approximately determined and allowed for[e]. We assume a surface of mercury to be perfectly plane, but even in the breadth of 5 inches there is a calculable divergence from a true plane of about one ten-millionth part of an inch; and this surface further diverges from true horizontality as the plumb-line does from true verticality. That most perfect instrument, the pendulum, is not even theoretically perfect, except for infinitely small arcs, and the delicate experiments performed with the torsion balance proceed on the assumption that the force of torsion of a wire is proportional to the angle of torsion, which is again only true for infinitely small angles[f].

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[e] Pratt, 'Philosophical Transactions,' vol. cxlvi. p. 31.

We need to take great care that in simplifying a problem we do not overlook some circumstance which from peculiar mathematical conditions is of importance. Thus in experiments upon the density of the earth we may treat irregularities of its contour as producing inconsiderable effects. But a like assumption must not be made concerning irregularities in the strata of the earth at a short distance below the point of experiment[g].

Such is the purely approximate character of all our operations that it is not uncommon to find the theoretically worse method giving truer results than the theoretically perfect method. The common pendulum which is not isochronous is better for practical purposes than the cycloidal pendulum which is isochronous in theory, but subject to mechanical difficulties. The spherical form is not the correct form for a speculum or lense, but it differs so slightly from the true form, and is so much more easily produced mechanically, that it is generally best to rest content with the spherical surface. Even in a six-feet mirror the difference between the parabola and the sphere is only about $\frac{1}{10,000}$ of an inch, a thickness which would be taken off in a few rubs of the polisher. Watts' ingenious parallel motion was intended to produce rectilinear movement of the piston rod. In reality the motion was always curvilinear, but a certain part of the curve approximated sufficiently for his purposes to a straight line.

Approximation to Exact Laws.

Though we can never prove any numerical law with perfect accuracy, it would be a great mistake to suppose that there is any inexactness in the laws of nature. We may even discover a law which we believe to represent the action of forces with perfect exactness. The mind may seem to pass in advance of its data, and choose out certain numerical results as absolutely true. We can never really pass beyond our data, and so far as assumption enters in, so far want of certainty will attach to our conclusions; nevertheless we may in many cases rightly prefer a probable assumption of a precise law to numerical results, which are at the best only approximative. We must accordingly draw a strong distinction between the laws of nature which we believe to be accurately stated in our formulas, and those to which our statements only make an approximation, so that at a future time the law will be differently stated.

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[f] Baily, 'Memoirs of the Royal Astronomical Society,' vol. xiv, p. 99.

[g] Airy, Philosophical Transactions,' vol. cxlv.

The law of gravitation is expressed in the form $F = \frac{Mm}{D^2}$, meaning that gravity is proportional directly to the product of the gravitating masses, and indirectly to the square of their distance. The latent heat of steam, again, is expressed by the equation $\log F = a + b\alpha^t + c\beta^t$, in which are five quantities a, b, c, α, β, to be determined by experiment. Now there is every reason to believe that in the progress of science the law of gravity will remain entirely unaltered, and the only effect of further inquiry will be to render it a more and more probable expression of the absolute truth. The law of the latent heat of steam, on the other hand, will be modified by every new series of experiments, and it may not improbably be shown that the assumed law can never be made to agree with the results of experiment.

Philosophers have by no means always supposed that the law of gravity was exactly true. Newton, though he had the highest confidence in its truth, admitted that there were motions in the planetary system which he could not reconcile with the law. Euler and Clairaut who were, with D'Alembert, the first to apply the full powers of mathematical analysis to the theory of gravitation as explaining the perturbations of the planets, did not treat the law as sufficiently established to attribute all discrepancies to the errors of calculation and observation. In short, they did not feel certain that the force of gravity exactly obeyed the well known rule. The law might have involved other powers of the distance. It might have been expressed, for example, in the form

$$F = \cdots + \frac{a}{D} + \frac{b}{D^2} + \frac{c}{D^3} + \cdots$$

and the coefficients a and c might have been so small that those terms would only become apparent in very accurate comparisons with fact. Attempts have been made from time to time to account for difficulties, by attributing value to such neglected terms. Gauss at one time thought that the even more fundamental principle of gravity, that the force is dependent only on mass and distance, might not be exactly true, and he undertook accurate pendulum experiments to test this opinion. Only as these repeated doubts have been time after time resolved in favour of the law of Newton, has it been assumed as precisely correct. But this belief does not rest on experiment or observation only. The calculations of physical astronomy, however accurate, could never show that the other terms of the above general expression were absolutely devoid of value. It could only be shown that they had such slight value as never to become apparent.

There are, however, other theoretical reasons why the law is probably complete and true as commonly stated. Whatever influence or power spreads from a point, and expands uniformly through space, will doubt
versely in intensity as the square of the dis
because the area over which it is spread ir

square of the radius. This part of the law of gravity may be considered as due to the properties of space, and there is a perfect analogy in this respect between gravity and all other *emanating* forces or substances, as was pointed out in a most comprehensive and clear manner by Keill[h]. Thus the undulations of light, heat, sound, and the attractions of electricity or magnetism obey the very same law so far as we can ascertain. If the molecules of a gas or the particles of matter constituting odour were to start from a point and move from it in straight lines uniformly, their distances would increase and their density decrease according to the same principles.

The other known laws of nature stand in a precisely similar position. Dalton's laws of definite combining proportions never have been, and never can be exactly proved; but chemists having shown, to a considerable degree of approximation, that all the more common elements combine together as if each element had atoms of an invariable mass, assume that this is exactly true. They go even further. Prout pointed out in 1815 that the equivalent weights of the elements appeared to be simple commensurable numbers; and Dumas, Pelouze, Marignac, Erdmann, Stas, and others have gradually rendered it likely that the atomic weights of hydrogen, carbon, oxygen, nitrogen, chlorine, and silver, are in the ratios of the numbers 1, 12, 16, 14, 35·5, and 108. Chemists then step beyond their data; they throw aside their actual experimental numbers, and assume that the true ratios are not those exactly indicated by any weighings, but the simple ratios of these numbers. They boldly assume that the discrepancies are due to experimental errors, and they are justified by the fact that the more elaborate and skilful the researches on the subject, the more nearly their assumption is verified.

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[h] 'An Introduction to Natural Philosophy,' 3rd. edit., 1733, p. 5.

Potassium is the only element whose atomic weight has been determined with great care, but which has not shown an approach to a simple ratio with the other elements. This exception may be due to some unsuspected cause of error[i]. A similar assumption is also made in the law of definite combining volumes of gases, and Sir B. C. Brodie has clearly pointed out the line of argument by which the chemist, observing that the discrepancies between the law and fact are within the limits of experimental error, assumes that they are due to error[k].

Faraday, in one of his researches, expressly makes an assumption of the same kind. Having shown, with some degree of experimental precision, that there exists a simple proportion between quantities of electrical energy and the quantities of chemical substances which it can decompose, so that for every atom dissolved in the battery cell an atom ought theoretically, that is without regard to dissipation of some of the energy, to be decomposed in the electrolytic cell, he does not stop at his numerical results. 'I have not hesitated,' he says, 'to apply the more strict results of chemical analysis to correct the numbers obtained as electrolytic results. This, it is evident, may be done in a great number of cases[l], without using too much liberty towards the due severity of scientific research.'

The law of the conservation of energy itself, one of the widest of all physical generalizations, must rest upon the same footing. The most that we can do by experiment is to show that the energy entering into any experimental combination is almost exactly equal to what comes out of it, and more nearly so the more accurately we perform all the measurements. Absolute equality is always a matter of assumption. We cannot even prove the indestructibility of matter; for were an exceedingly minute fraction of existing matter to vanish in any experiment, say one part in ten millions, we could never detect the loss.

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[i] Watts, 'Dictionary of Chemistry,' vol. i. p. 455.

[k] 'Philosophical Transactions,' (1866) vol. clvi. p. 809.

[l] 'Experimental Researches in Electricity,' vol. i. p. 246.

Successive Approximations to Natural Conditions.

When we examine the history of scientific problems, we find that one man or one generation is usually able to make but a single step at a time. A problem is always solved for the first time by making some bold hypothetical simplification, upon which the next investigator makes hypothetical modifications approaching more nearly to the truth. Errors are successively pointed out in previous solutions, until at last there might seem little more to be desired. Careful examination, however, will show that an indefinite series of minor inaccuracies remain to be corrected and explained, were our powers of reasoning sufficiently great, and the purpose adequate in importance.

Newton's successful solution of the problem of the planetary movements entirely depended at first upon a great but hypothetical simplification. The law of gravity only applies directly to two infinitely small particles, so that when we deal with vast globes like the earth, Jupiter, or the sun, we have an immense aggregate of separate attractions to deal with, and the law of the aggregate need not coincide with the law of the elementary particles. But Newton, by a great effort of mathematical reasoning, was able to show that two homogeneous spheres of matter act as if the whole of their masses were concentrated at the centres; in short, that such spheres are aggregates which manifest the simple law of gravity or are centrobaric bodies (vol. i. p. 423). He was then able with comparative ease to calculate the motions of the planets on the hypothesis of their being spheres, and to show that the results roughly agreed with observation.

Newton, indeed, was one of the few men who could make two great steps at once. He did not rest contented with the spherical hypothesis; having reason to believe that the earth was really a spheroid with a protuberance around the equator, he proceeded to a second approximation, and proved that the attraction of the protuberant matter upon the moon accounted for the precession of the equinoxes, and led to various complicated effects. But, as I have already mentioned (vol. ii. p. 76), even the spheroidal hypothesis is far from the truth. It takes no account of the irregularities of surface, the great protuberance of land, for instance, in Central Asia and South America, and the deficiency in the bed of the Atlantic.

To determine the law according to which a projectile, such as a cannon ball, moves through the resisting atmosphere is a problem very imperfectly solved at the present day, but in which many successive advances have been made. So little was known concerning the subject three or four centuries ago that a cannon ball was supposed to move at first in a straight line, and only after a time to be deflected into a curve. Tartaglia ventured to maintain that the path was curved throughout, as by the principle of continuity it should be; but the ingenuity of Galileo was required to prove this opinion, and to show that the curve was approximately a parabola. It is only, however, under several forced hypotheses that we can assert the path of a projectile to be truly a parabola: the path must be through a perfect vacuum, where there is no resisting medium of any kind; the force of gravity must be equal and act in parallel lines; and the moving body must be either a mere point, or a perfect centrobaric body, that is a body possessing a definite centre of gravity. None of these conditions can be really fulfilled in practice. The next great step in the problem was made by Newton and Huyghens, the latter of whom asserted that the atmosphere would offer a resistance proportional to the velocity of the moving body, and concluded that the path would have in consequence a logarithmic character. Newton investigated in a general manner the subject of resisting media, and came to the conclusion that the resistance was more nearly proportional to the square of the velocity. The subject then fell into the hands of Daniel Bernouilli, who pointed out the enormous resistance of the air in cases of rapid movement, and calculated that a cannon ball, if fired vertically in a vacuum, would rise eight times as high as in the atmosphere. In more recent times an immense amount both of theoretical and experimental investigation has been spent upon the subject, since it is one of great importance in the art of war. Successive approximations to the true law have been made, but nothing like a complete and final solution has been achieved or even hoped for[m].

It is quite to be expected that the earliest experimenters in any branch of science will overlook corrections which afterwards become most apparent. The Arabian astronomers determined the meridian by taking the middle point between the places of the sun when at equal altitudes on the same day. They overlooked the fact that the sun has its own motion among the stars in the time intervening between the observations. Newton thought that the mutual disturbances of the planets might be disregarded, excepting perhaps the effect of the mutual attraction of the greater planets, Jupiter and Saturn, near their conjunction[n]. The expansion of quicksilver was long used as the measure of temperature, in ignorance or disregard of the fact that the rate of expansion increases with the temperature. Rumford, in the first experiment leading to a determination of the mechanical equivalent of heat, disregarded the heat absorbed by the box containing the water heated and by other parts of the apparatus, otherwise he would in Dr. Joule's opinion, have come nearly to the correct result.

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[m] Hutton's 'Mathematical Dictionary,' vol. ii. pp. 287–292.

[n] 'Principia,' bk. iii. Prop. 13.

It is surprising to learn the number of causes of error which enter into even the simplest experiment, when we strive to attain the most rigid accuracy. Thus we cannot perform the simple experiment of compressing a portion of gas in a bent tube by a column of mercury, in order to test the truth of Boyle's Law, without paying regard to,—(1) the variations of atmospheric pressure, which are communicated to the gas through the mercury; (2) the compressibility of mercury, which causes the column of mercury to vary in density; (3) the temperature of the mercury throughout the column; (4) the temperature of the gas which is with difficulty maintained invariable; (5) the expansion of the glass tube containing the gas. Although Regnault took all these circumstances into account in his accurate examination of the law[o], there is no reason for supposing that he exhausted the sources of inaccuracy.

All the earlier investigations concerning the nature of waves in elastic media proceeded upon the assumption that waves of different length would travel with equal speed. Newton's theory of sound had led him to this conclusion, and experiment, or indeed the commonest observations (see vol. i. p. 344) had sufficiently verified the inference. When the undulatory theory came to be applied at the commencement of this century to explain the phenomena of light, a great difficulty was encountered. The angle at which a ray of light is refracted in entering a denser medium depends, according to that theory, on the velocity with which the wave travels, so that if all waves of light were to travel with equal velocity in the same

[o] Jamin, 'Cours de Physique,' vol. i. pp. 282–3

medium, the dispersion of mixed light by the prism and the production of the spectrum could not take place. Some of the most striking phenomena were thus in direct conflict with the theory. The great French mathematician, Cauchy, first pointed out the true explanation, namely that all previous investigators had made an arbitrary assumption for the sake of simplifying the calculations. They had assumed that the particles of the vibrating medium are so close together that the intervals are quite inconsiderable compared with the length of the wave, or in other terms infinitely small. This hypothesis happened to be approximately true in the case of air, so that no error was discovered in experiments on sound. Had it not been so, the earlier analysts would probably have failed to give any solution, and the progress of the subject might have been retarded. Cauchy was able to make a new approximation to truth under the more difficult supposition, that the particles of the vibrating medium are situated at considerable distances, and act and react upon the neighbouring particles by attractive and repulsive forces. To calculate the rate of propagation of a disturbance in such a medium is a work of excessive difficulty. The complete solution of the problem appears indeed to be beyond human power, so that we must be content, as in the case of the planetary motions, to look forward to successive approximations. All that Cauchy could do was to show that certain mathematical terms or quantities, neglected in previous theories, became of considerable amount under the new conditions of the problem, so that there will exist a relation between the length of the wave, and the velocity at which it travels. To remove, then, the difficulties in the way of the undulatory theory of light, a new approach to probable conditions was needed[p].

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[p] Lloyd's 'Lectures on the Wave Theory,' pp. 22, 23.

In a similar manner Fourier's theory of the conduction and radiation of heat was based upon the hypothesis that the quantity of heat passing along any line is simply proportional to the rate of change of temperature. But it has since been shown by Forbes that the conductivity of a body diminishes as its temperature increases. All the details of Fourier's solution therefore require modification, and the results are in the meantime to be regarded as only approximately true[q].

We ought to distinguish between those problems which are physically and those which are merely mathematically incomplete. In the latter case the physical law is correctly seized, but the mathematician neglects, or is more often unable to follow out the law in all its results. The law of gravitation and the principles of harmonic or undulatory movement, even supposing the data to be correct, can never be followed into all their ultimate results. Dr. Young explained the production of Newton's rings by supposing that the rays reflected from the upper and lower surfaces of a thin film of a certain thickness were in opposite phases, and thus neutralized each other. It was pointed out, however, that as the light reflected from the nearer surface must be undoubtedly a little brighter than that from the further surface, the two rays ought not to neutralize each other so completely as they are observed to do. It was finally shown by Poisson that the discrepancy arose only from incomplete solution of the problem; for the light which has once got into the film must be to a certain extent reflected backwards and forwards *ad infinitum*; and if we follow out this course of the light by a perfect mathematical analysis, absolute darkness may be shown to result from the interference of the rays[r]. In such a case as this we used no physical laws but those of reflection and refraction, and the only difficulty consisted in developing their full consequences.

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[q] Tait's 'Thermodynamics,' p. 10.

[r] Lloyd's 'Lectures on the Wave Theory,' pp. 82, 83.

There is one instructive result of the theory of error which should always be borne in mind, namely that when a large variable error is combined with a small variable error, the uncertainty of the final result, as measured by its probable error, is scarcely at all affected by the small variable error[s]. Accordingly our efforts at accuracy must be devoted to the sources of error in the order of their magnitude. There is no use in making instruments to measure the heat of the sun with the last degree of accuracy, when the varying transparency of the atmosphere produces uncertainties of far greater amount. It is needless to observe a comet or other heavenly body with the very finest instruments if it appears low down on the horizon, where the atmospheric refraction is not accurately determinate. In short, minuter *variable* sources of error may be entirely neglected, so long as those of a considerably greater amount remain beyond our powers of correction.

Discovery of Hypothetically Simple Laws.

In some branches of science we meet with natural laws of a simple character which are in a certain point of view exactly true and yet can never be manifested as exactly true in natural phenomena. Such, for instance, are the laws concerning what is called a *perfect gas.* The gaseous state of matter is that in which the general properties of matter are exhibited in the simplest and most general manner. There is much advantage accordingly in approaching the question of molecular mechanics from this side. But when we ask the question—What is a gas? the answer must be a hypothetical one. Finding that gases *nearly* obey the law of Boyle and Marriotte; that they *nearly* expand by heat at the uniform rate of one part in 272·9 of their volume at 0° for each degree centigrade; and that they *more nearly* fulfil these conditions the more distant the point of temperature at which we examine them from the liquefying point, we pass by the principle of continuity to the conception of a perfect gas. Such a gas would probably consist of atoms of matter at so great a distance from each other as to exert no attractive forces upon each other; but for this condition to be exactly fulfilled the distances must be infinite, so that an absolutely perfect gas cannot exist. But the perfect gas is not merely a limit to which we may approach, it is a limit passed by at least one real gas. It has been shown by Despretz, Pouillet, Dulong, Arago, and finally Regnault, that all gases diverge from the Boylean law, and in nearly all cases the density of the gas increases in a somewhat greater ratio than the pressure, indicating a tendency on the part of the molecules to approximate of their own accord, and condense into liquid. In the more condensible gases such as sulphurous acid, ammonia, and cyanogen, this tendency is strongly apparent near the liquefying point. Hydrogen on the contrary diverges from the law of a perfect gas in the opposite direction, that is, the density increases less than in the ratio of the pressure[t]. This is a singular exception, the bearing of which I am unable to comprehend.

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[s] Airy, 'Philosophical Transactions,' (1856) vol. cxlvi. p. 324.

All gases diverge again from the law of uniform expansion by heat, but the divergence is less as the gas in question is less condensible, or examined at a temperature more removed from its liquefying point. Thus the perfect gas in this respect must have an infinitely high temperature. According to Dalton's law each gas in a mixture retains its own properties wholly unaffected by the presence of any other gas[u]. This law is probably true only by approximation, but it is obvious that it would be true of the perfect gas with infinitely distant particles[x].

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[t] Jamin, 'Cours de Physique,' vol. i. pp. 283–288.

Mathematical Principles of Approximation.

The whole subject of the approximate character of physical science will be rendered more plain if we consider it from a general mathematical point of view. Throughout quantitative investigations we deal with the relation of one quantity to certain other quantities, of which it is a function; but the subject is quite sufficiently complicated if we view one quantity as a function of one other. Now, as a general rule, a function can be developed or expressed as the sum of certain other quantities, the values of which depend upon the successive powers of the variable quantity. Thus, if y be the one quantity which is regarded as a function of x, then we may say that

$$y = \mathrm{A} + \mathrm{B}x + \mathrm{C}x^2 + \mathrm{D}x^3 + \mathrm{E}x^4 + \ldots .$$

In this equation, A, B, C, D, &c., are fixed quantities, of different values in different cases. The terms may be infinite in number or after a time may cease to have any value. Any of the co-efficients A, B, C, &c., may be zero or negative; but whatever they may be they are fixed. The quantity x on the other hand may be made what we like, being variable at our will. Suppose, in the first place, that x and y are both measurable lengths. Let us assume that $\frac{1}{10,000}$ part of an inch is the least that we can take note of. Then when x is one hundredth of an inch, we have $x^2 = \frac{1}{10,000}$, and if C be less than unity, the term $\mathrm{C}x^2$ will be inappreciable, being less than we can measure. Unless any of the quantities D, E, &c., should happen to be very great, it is evident that all the succeeding terms will also be inappreciable, because the powers of x become rapidly smaller in geometrical ratio. Thus when x is made small enough the quantity y seems to obey the equation

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[u] Joule and Thomson, 'Philosophical Transactions,' 1854, vol. cxliv. p. 337.

[x] The properties of a perfect gas have been described by Rankine, 'Transactions of the Royal Society of Edinburgh,' vol. xxv. p. 561.

$$y = A + Bx.$$

If x should be made still less, if it should become so small, for instance, as $\frac{1}{1.000.000}$ of an inch, and B should not be very great, then y would appear to be the fixed quantity A, and would not seem to vary with x at all. On the other hand, were x to grow greater, say equal to $\frac{1}{10}$ inch, and C not be very small, the term Cx^2 would become appreciable, and the law would now be more complicated.

We can invert the mode of viewing this question, and suppose that while the quantity y undergoes variations depending on many powers of x, that our power of detecting the changes of value is more or less acute. While our powers of observation remain very rude and imperfect we may even be unable to detect any change in the quantity at all, that is to say Bx may always be smaller than to come within our notice, just as in former days the fixed stars were so called because they remained at apparently fixed distances from each other. With the use of telescopes and micrometers we become able to detect the existence of some motion, so that the distance of one star from another may be expressed by $A + Bx$, the term including x^2 being still inappreciable. Under these circumstances the star will seem to move uniformly, or in simple proportion to the time, x. With much improved means of measurement it will probably be found that this uniformity of motion is only apparent, and that there exists some acceleration or retardation due to the next term. *More and more* careful investigation will show the law to be more and more complicated than was previously supposed.

There is yet another way of explaining the apparent results of a complicated law. If we take any curve and regard only a portion of it free from any kind of discontinuity, we may represent the character of such portion by an equation of the form

$$y = \mathrm{A} + \mathrm{B}x + \mathrm{C}x^2 + \mathrm{D}x^3 + \ldots\ldots$$

Restrict the attention to a very small portion of the curve, and the eye will be unable to distinguish its difference from a straight line, which amounts to saying that in the portion examined the term $\mathrm{C}x^2$ has no value appreciable by the eye. Take a larger portion of the curve and it will be apparent that it possesses curvature, but it will be possible to draw a parabola or ellipse so that the curve shall be apparently coincident with a portion of that parabola or ellipse. In the same way if we take larger and larger arcs of the curve it will assume the character successively of a curve of the third and fourth degrees; that is to say, it corresponds to equations involving the third and fourth powers of the variable quantity.

We have arrived then at the conclusion that every phenomenon, when its amount can only be rudely measured, will either be of fixed amount, or will seem to vary uniformly like the distance between two inclined straight lines. More exact measurement may show the error of this first assumption, and the variation will then appear to be like that of the distance between a straight line and a parabola or ellipse. We may afterwards find that a curve of the third or higher degrees is really required to represent the variation. I propose to call the variation of a quantity *linear*, *elliptic*, *cubic*, *quartic*, *quintic*, &c., according as it is discovered to involve the first, second, third, fourth, fifth or higher powers of the variable. It is a general rule in quantitative investigation that we commence by discovering linear, and afterwards proceed to elliptic or more complicated laws of variation. The approximate curves which we employ are all, according to De Morgan's use of the name, parabolas of some order or other; and since the common parabola of the second order is approximately the same as a very elongated ellipse, and is in fact an infinitely elongated ellipse, it is convenient and proper to call variation of the second order *elliptic.* It might also be called *quadric* variation.

As regards many important phenomena we are yet only in the first stage of approximation. We know that the sun and many so-called fixed stars, especially 61 Cygni, have a proper motion through space, and the direction of this motion at the present time is known with some degree of accuracy. But it is hardly consistent with the theory of gravity that the path of any body should really be a straight line. Hence, we must regard a rectilinear path as only an approximate and provisional description of the motion, and look forward to the time when its curvature will be ultimately detected and measured, though centuries perhaps must first elapse.

On the surface of the earth we are accustomed to assume that the force of gravity is uniform at all ordinary heights above or below the surface, because the variation is of so slight an amount that we are scarcely able to detect it. But supposing we could measure the variation, we should find it simply proportional to the height. Taking the earth's radius to be unity, let h be the height at which we measure the force of gravity. Then by the well-known law of the inverse square, that force will be proportional to

$$\frac{g}{(1+h)^2}, \text{ or to } g\,(1 - 2\,h + 3\,h^2 - 4\,h^3 + \ldots\ldots).$$

But at all heights to which we can attain h will be so small a fraction of the earth's radius that $3\,h^2$ will be inappreciable, and the force of gravity will seem to follow the law of linear variation, being proportional to $1-2h$.

When the circumstances of an experiment are much altered, different powers of the variable may become prominent. The resistance of a liquid to a body moving through it may be approximately expressed as the sum of two terms respectively involving the first and second powers of the velocity. At very low velocities the first power is of most importance, and the resistance, as Professor Stokes has shown, is nearly in simple proportion to the velocity. When the motion is rapid the resistance increases in a still greater degree, and is more nearly proportional to the square of the velocity.

Approximate Independence of Small Effects.

One result of the general theory of approximation possesses such great importance in physical science, and is so often applied, that we may consider it separately. The investigation of causes and effects is immensely simplified when we may consider each cause as producing its own effect invariably, whether other causes are acting or not. Thus, if the body P produces the effect x, and Q produces y, the question is whether P and Q acting together will produce simply the sum of the separate effects, $x+y$. It is under this supposition that we treated the methods of eliminating error (Chap. XV.), and errors of a less amount would still remain if the supposition was a forced and unnatural one. There are probably some parts of science in which the supposition of independence of effects holds rigidly true. The mutual gravity of two bodies, for instance, is entirely unaffected by the presence or absence of other gravitating bodies. People do not usually consider that this important principle is involved in such a simple thing as putting two pound weights in the scale of a balance. How do we know that two pound weights together will weigh twice as much as one? Do we know it to be exactly so? Like other results founded on induction we cannot prove it certainly and absolutely, but all the calculations of physical astronomy proceed upon the assumption, so that we may consider it proved to a very high degree of approximation. We may, in fact, assume with much probability that bodies gravitate in entire independence of each other. Had not this been true the calculations of physical astronomy would have been almost infinitely more complex than they actually are, and the progress of knowledge would have been vastly slower.

The science of the spectrum again is much simplified by the fact that elements do not apparently interfere with each other in the production of light. The spectrum of sodium chloride is the spectrum of sodium superposed upon that of chlorine. Were it otherwise, we should have as many distinct spectra as there are distinct compounds in chemistry, and the subject would be almost hopelessly complex. The spectrum of a substance would then no more enable us to tell its components than the appearance of a new mineral indicates its composition. But it would probably be too early to assert the entire absence of any joint spectra. There is so much yet unexplained in the subject that some effects due to the mutual action of elements may possibly be discovered, and the independence will then be only approximate.

It is a general principle of scientific method that if effects be of small amount, comparatively to our means of observation, all joint effects will be of a higher order of smallness, and may therefore be rejected in a first approximation. This principle was distinctly employed by Daniel Bernouilli in the theory of sound, under the title of 'The Principle of the Coexistence of Small Vibrations.'

He showed that if a string is affected by two kinds of vibrations, we may consider each to be going on as if the other did not exist. We cannot perceive that the sounding of one musical instrument prevents or even modifies the sound of another, so that all sounds would seem to travel through the air, and act upon the ear in independence of each other. An exactly similar assumption is made in the theory of tides, which are really great waves. One wave is produced by the attraction of the moon, and another by the attraction of the sun, and the question arises, whether when these waves coincide, as at the time of spring tides, the joint wave will be simply the sum of the separate waves. On the principle of Bernouilli this will be so, because the tides on the ocean are almost indefinitely small compared with the depth of the ocean.

The principle of Bernouilli, however, is only approximately true. A wave never is exactly the same when another wave is interfering with it, but the less the displacement of particles due to each wave, the less in a still higher degree is the effect of one wave upon the other. In recent years Helmholtz was led to suspect that some of the phenomena of sound might after all be due to resultant effects overlooked by the assumption of previous physicists. He investigated the secondary waves which would arise from the interference of considerable disturbances, and was able to show that certain summation or resultant tones ought to be heard, and experiments subsequently devised for the purpose showed that they might be heard.

Throughout the mechanical sciences the *Principle of the Superposition of Small Motions* is of fundamental importance[y], and it may be thus explained. Suppose that two forces, acting from the points B and C, are simultaneously moving a body A. Let the force acting from B be such that in one second it would move A to p, and similarly let the second force, acting alone, move A to r. The question arises, then, whether their joint action will urge A to q along the diagonal of the parallelogram. May we say that A will move the distance Ap in the direction AB, and Ar in the direction AC, or, what is the same thing, along the parallel line pq? In all strictness we cannot say so; for when A has moved towards p, the force from C will no longer act along the line AC, and similarly the motion of A towards r will modify the action of the force from B. This interference of one force with the line of action of the other will evidently be greater the larger is the extent of motion considered; on the other hand, as we reduce the parallelogram Apqr, compared with the distances AB and AC, the less will be the interference of the forces. Accordingly mathematicians avoid all error by considering the motions as infinitely small, so that the interference becomes of a still higher order of infinite smallness, and may be entirely neglected. By the resources of the Differential Calculus it is possible to calculate the motion of the particle A, as if it went through an infinite number of infinitely small diagonals of parallelograms. The great discoveries of Newton really arose from applying this method of calculation to the movements of the moon round the earth, which, while constantly tending to move onward in a straight line, is also deflected towards the earth by gravity, and moves through an elliptic curve, composed as it were of the infinitely small diagonals of infinitely small parallelograms. The mathematician, in his investigation of a curve, always in fact treats it as made up of a great number of short straight lines, and it

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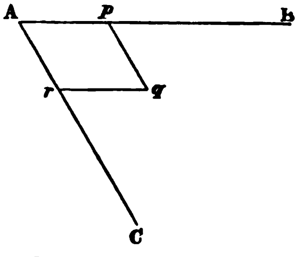
[y] See Thomson and Tait's 'Natural Philosophy,' vol. i. p. 60.



may even be doubtful whether he could treat it in any other manner. Nevertheless there is no error in the final results, because having obtained the formulæ flowing from this supposition, each straight line is then regarded as becoming infinitely small, and the polygonal line becomes undistinguishable from a perfect curve[z].

In abstract mathematical theorems the approximation to absolute truth is perfect, because we can treat of infinitesimals. In physical science, on the contrary, we treat of the least quantities which are perceptible. Nevertheless, while carefully distinguishing between these two different cases, we may fearlessly apply to both the principle of the superposition of small motions or effects. In physical science we have only to take care that the effects really are so small that any joint effect will be unquestionably imperceptible. Suppose, for instance, that there is some cause which alters the dimensions of a body in the ratio of 1 to $1 + \alpha$, and another cause which produces an alteration in the ratio of 1 to $1 + \beta$. If they both act at once the change will be in the ratio of 1 to $(1 + \alpha)(1 + \beta)$, or as 1 to $1 + \alpha + \beta + \alpha\beta$. But if α and β be both very small fractions of the total dimensions, $\alpha\beta$ will be yet far smaller and may be disregarded; the ratio of change is then approximately that of 1 to $1 + \alpha + \beta$, or the joint effect is the sum of the separate effects. Thus if a body were subjected to three strains at right angles to each other, the total change in the volume of the body would be approximately equal to the sum of the changes produced by the separate strains, provided that these are of very small amount. In like manner not only is the expansion of every solid and liquid substance by heat approximately proportional to the change of temperature, when this change is very small in amount, but the cubic expansion may also be considered as being three times as great as the linear expansion. For if the increase of temperature expands a bar of metal in the ratio of 1 to $1 + a$, and the expansion be equal in all directions, then a cube of the same metal would expand as 1 to $(1 + a)^3$, or as 1 to $1 + 3a + 3a^2 + a^3$. When a is a very small quantity the third term $3a^2$ will be imperceptible, and still more so the fourth term a^3. The coefficients of expansion of solids are in fact so small, and so imperfectly determined, that physicists seldom take into account their second and higher powers.

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[z] *Challis*, 'Notes on the Principles of Pure and Applied Calculation,' *1869, p. 83.*

It is an universal and important result of these principles that all very small errors may be assumed to vary in simple proportion to their causes; a new reason why, in eliminating errors, we should first of all make them as small as possible. Let us suppose, with De Morgan, that there is a right-angled triangle of which the two sides containing the right angle are really of the lengths 3 and 4, so that the hypothenuse is $\sqrt{3^2 + 4^2}$ or 5. Now if in two measurements of the first side we commit slight errors, making it successively 4·001 and 4·002, then calculation will give the lengths of the hypothenuse as almost exactly 5·0008 and 5·00016, so that the error in the hypothenuse will seem to vary in simple proportion to that of the side, although it does not really do so with perfect exactness[a]. The logarithm of a number does not vary in proportion to that number—nevertheless we should find the difference between the logarithms of the numbers 100000 and 100001 to be almost exactly equal to that between the numbers 100001 and 100002. It is thus a general rule that very small differences between successive values of a function are approximately proportional to the small differences of the variable quantity.

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[a] De Morgan's 'Differential Calculus.'

Four Meanings of Equality.

Although it might seem that there are few terms more free from ambiguity than the term *equal*, yet scientific men do as a matter of fact employ it with four meanings, which it is very desirable to distinguish carefully. These meanings I may briefly describe as

(1) Absolute Equality.
(2) Sub-equality.
(3) Apparent Equality.
(4) Probable Equality.

By *absolute equality* we signify that which is complete and perfect to the last degree; but it is obvious that we can only know such equality in a theoretical or hypothetical manner. The areas of two triangles standing upon the same base and between the same parallels are absolutely equal. Hippocrates beautifully proved that the area of a lunula or figure contained between two segments of circles was absolutely equal to that of a certain right-angled triangle. As a general rule all geometrical and other elementary mathematical theorems involve absolute equality.

De Morgan proposed to describe as *sub-equal* those quantities which are equal within an infinitely small quantity, so that x is sub-equal to $x + dx$. The whole of the differential calculus may, as I apprehend it, be said to arise out of the neglect of infinitely small quantities; with this subject however we are not in this place much concerned. In mathematical science many other subtle distinctions may have to be drawn between kinds of equality, as De Morgan has shown in a remarkable memoir 'On Infinity; and on the Sign of Equality'[b].

Apparent equality is that with which physical science deals. Those magnitudes are practically equal which differ only by an imperceptible quantity. To the carpenter anything less than the hundredth part of an inch is non-existent; there are few arts or artists to which the hundred-thousandth of an inch is of any account. Since all coincidence between physical magnitudes is judged by one or other sense, we must be restricted to a knowledge of apparent equality.

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[b] 'Cambridge Philosophical Transactions,' (1865) vol. xi, Part I.

In reality even apparent equality is rarely to be expected. More commonly experiments will give only *probable equality*, that is results will come so near to each other that the difference may be ascribed to unimportant disturbing causes. Thus physicists often assume quantities to be equal provided that they fall within the limits of probable error of the processes employed. We cannot expect observations to agree with theory more closely than they agree with each other, as Newton remarked of his investigations concerning Halley's Comet.

Arithmetic of Approximate Quantities.

Considering that almost all the quantities which we treat in physical and social science are approximate only, it seems desirable that some attention should be paid in the teaching of arithmetic to the correct interpretation and treatment of approximate numerical statements. We ought carefully to distinguish between 2·5 when it means exactly two and a half, and when it means, as it usually does, anything between 2·45 and 2·55 It would be better in the latter case to write the number as 2·5 . . . . and we might then distinguish 2·50 . . . . as meaning anything between 2·495 . . . . and 2·505. When approximate numbers are added, subtracted, multiplied, or divided, it becomes a matter of some complexity to determine the degree of accuracy of the result. There are few persons, for instance, who could assert straightway that the sum of the approximate numbers 34·70, 52·693, 80·1, is 167·5 *within less than* ·07. So far as I know Mr. Sandeman is the only mathematician who has traced out the rules of approximate arithmetic, and his directions are worthy of careful attention[c]. Although the accuracy of measurement has so much advanced since the time of Leslie, it is not superfluous to repeat his protest against the unfairness of affecting by a display of decimal fractions a greater degree of accuracy than the nature of the case requires and admits[d]. I have known a scientific man to register the barometer to a second of time when the nearest quarter of an hour would have been amply sufficient. Chemists often publish results of analysis to the ten-thousandth or even the millionth part of the whole, when in all probability the processes employed cannot be depended on beyond the hundredth part. It is seldom desirable to give more than one place of figures of uncertain amount; but it must be allowed that a nice perception of the degree of accuracy possible and desirable is requisite to save misapprehension and needless computation on the one hand, and to secure all attainable exactness on the other hand.

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[c] Sandeman, 'Pelicotetics,' p. 214.

[d] Leslie, 'Inquiry into the Nature of Heat,' p. 505.

CHAPTER XXII.

QUANTITATIVE INDUCTION.

LET it be observed that we have not yet formally considered any processes of reasoning which have for their object to disclose general laws of nature expressed in quantitative formulæ or equations. We have been inquiring into the modes by which a phenomenon may be measured, and, if it be a composite phenomenon, may be resolved, by the aid of several measurements, into its component parts. We have also considered the precautions to be taken in the performance of observations and experiments in order that we may know what phenomena we really do measure and record. In treating of the approximate character of all observations, we have partially entered upon the subject of Quantitative Induction proper, but we must remember that no number of facts and observations can by themselves constitute science or general knowledge. Numerical facts, like other facts, are but the raw materials of knowledge, upon which our reasoning faculties must be exerted in order to draw forth the secret principles of nature. It is by an inverse process of reasoning that we can alone discover the mathematical laws to which varying quantities conform. By well-conducted experiments we gain a series of values of a variable, and a corresponding series of values of a variant, and we now want to know what mathematical function the variant is as regards the variable. In the usual progress of a science three questions will have to be answered as regards every *important* quantitative phenomenon:—

(1) Is there any constant relation between the variable and variant?

(2) What is the empirical formula expressing this relation?

(3) What is the rational formula expressing the law of nature involved?

Probable Connexion of Varying Quantities.

We find it stated in Mr. Mill's System of Logic[a] that 'Whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation.' This assertion may be considered true when it is interpreted with sufficient caution; but it might otherwise lead us into great errors. There is nothing whatever in the nature of things to prevent the existence of two variations which should apparently follow the same law, and yet have no connexion with each other. One binary star might be going through a revolution which, so far as we could tell, was of apparently equal period with that of another binary star, and according to the above rule the motion of one would be the cause of the motion of the other, which would not be really the case. Two astronomical clocks might conceivably be made so nearly perfect that, for several years, no difference could be detected, and we might then infer that the motion of one clock was the cause or effect of the motion of the other. This matter really requires the most careful discrimination. We must always bear in mind that the continuous quantities of space, time, force, &c., which we measure, are made up of an infinite number of infinitely small units. We may then meet with two variable phenomena which follow laws so nearly the same, that in no part of the variations open to our observation can any discrepancy be discovered. I grant that if two clocks could be shown to have kept *exactly* the same time during one year, or any finite interval of time, the probability would become infinitely high that there was a connexion between their motions. But it is apparent that we can never absolutely prove such coincidences to exist. Allow that we may observe a difference of one tenth or one hundredth of a second in their time, yet it is just possible that they were independently regulated so as to go together within less than that quantity of time. In short it would require either an infinitely long time of observation, or infinitely acute powers of measuring a discrepancy to decide positively whether two clocks were or were not in relation with each other.

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[a] Book iii. chap. viii, § 6.

A similar question actually occurs in the case of the moon's motion. We have absolutely no record that any other portion of the moon was ever visible to men than such as we now see. This fact sufficiently proves that within the historical period the rotation of the moon on its own axis has coincided with its revolutions round the earth. Does this coincidence prove a relation of cause and effect to exist between these motions? The answer must be in the negative, because there might have been so slight a discrepancy between the motions that there has not yet been time to produce any appreciable effect. There may nevertheless be a high probability of connexion.

The whole question of the relation of quantities thus resolves itself into one of probability. When we can only rudely measure a quantitative result, we can assign but slight importance to any correspondence. Because the brightness of two stars seems to vary in the same manner there is no appreciable probability that they have any relation with each other. Could it be shown that their periods of variation were the same even to infinitely small quantities it would be certain, that is infinitely probable, that they were connected, however unlikely this might be on other grounds. The general mode of estimating such probabilities is identical with that applied to other inductive problems. Thus, if the two periods of variation were assigned by pure chance and entirely independently of each other, the probability would be about one in ten million that they would agree to the one ten-millionth part; but if the periods be observed to agree to less than that part then there is a probability of at least ten million to one in favour of the opposite hypothesis of connexion. That any two periods of variation should by chance become *absolutely equal* is infinitely improbable; hence if, in the case of the moon or any other change, we could prove absolute coincidence, we should have certainty of connexion[b]. With approximate measurements, which alone are within our power, we must hope for approximate certainty at the most.

The general principles of inference and probability, according to which we treat causes and effects varying in amount, are exactly the same as those by which we treated simple experiments. Continuous quantity, however, affords us an infinitely more extensive sphere of observation, because every different amount of cause, however little different, ought to be followed by a different amount of effect. If we can measure temperature to the one hundredth part of a degree centigrade, then even between 0° and 100° we have 10,000 possible distinct trials. If the precision of our measurements is increased, so that the one thousandth part of a degree can be appreciated, our trials may be increased tenfold. The probability of connexion will be proportional to the accuracy of our measurements.

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[b] *Laplace*, 'System of the World,' transl. by Harte, vol. ii. p. 366.

When we have the power of varying the quantity of a cause entirely at our will it is easy to discover whether a certain effect is due to that cause or not. We can then make as many regular or irregular changes as we like, and it is quite incredible that the supposed effect should by chance go through exactly the corresponding series of changes unless by dependence. Thus, if we have a bell ringing *in vacuo*, the sound increases as we let in the air, and it decreases again as we exhaust the air. Tyndall's singing flames evidently obeyed the directions of his own voice; and Faraday when he discovered the relation of magnetism and light found that, by making or breaking or reversing the current of the electro-magnet, he had complete command over a ray of light, proving beyond all reasonable doubt the dependence of cause and effect. In such cases it is the perfect coincidence in time between the change in the effect and that in the cause which raises a high improbability of casual coincidence.

It is by a very simple case of variation that we infer the existence of a material connexion between two bodies moving with exactly equal velocity, such as the locomotive engine and the train which follows it. Elaborate observations were requisite before astronomers could all be convinced that the red hydrogen flames seen during solar eclipses belonged to the sun, and not to the moon's atmosphere as Flamsteed assumed. As early as 1706, Captain Stannyan noticed a blood red streak in an eclipse which he witnessed at Berne, and he asserted that it belonged to the sun; but his opinion was not finally established until photographs of the eclipse in 1860, taken by Mr. De la Rue, showed that the moon's dark body gradually covered the red prominences on one side, and uncovered those on the other, in short, that these prominences moved precisely as the sun moved and not as the moon moved.

Even when we have no means of accurately measuring the variable quantities we may yet be convinced of their connexion, if one always varies perceptibly at the same time as the other. Fatigue increases with exertion; hunger with abstinence from food; desire and degree of utility decrease with the quantity of commodity consumed. We know that the sun's heating power depends upon his height in the sky; that the temperature of the air falls in ascending a mountain; that the earth's crust is found to be perceptibly warmer as we sink mines into it; we infer the direction in which a sound comes from the change of loudness as we approach or recede. The facility with which we can time after time observe the increase or decrease of one quantity with another sufficiently shows the connexion, although we may be unable to assign any precise law of relation. The probability in such cases depends upon frequent coincidence in time.

Empirical Mathematical Laws.

It is important to acquire a clear comprehension of the part which is played in scientific investigation by empirical formulæ and laws. If we have a table containing certain values of a variable and the corresponding values of the variant, there are certain mathematical processes by which we can infallibly discover a mathematical formula yielding numbers in more or less exact agreement with the table. We may generally assume that the quantities will approximately conform to a law of the form

$$y = A + Bx + Cx^2,$$

in which x is the variable and y the variant. We can then select from the table three values of y, and the corresponding values of x; inserting them in the equation, we obtain three equations by the solution of which we gain the values of A, B, and C. It will be found as a general rule that the formula thus obtained yields the other numbers of the table to a considerable degree of approximation.

In many cases even the second power of the variable will be unnecessary; thus Regnault found that the results of his elaborate inquiry into the latent heat of steam at different pressures were represented with sufficient accuracy by the empirical formula

$$\lambda = 606 \cdot 5 + 0 \cdot 305\, t,$$

in which λ is the total heat of the steam, and t the temperature[c]. In other cases it may be requisite to include the third power of the variable. Thus physicists assume the law of the dilatation of liquids to be of the form

$$\delta_t = at + bt^2 + ct^3,$$

and they calculate from results of observation the values of the three constants a, b, c, which are usually small quantities not exceeding one hundredth part of a unit, but requiring to be determined with great accuracy[d]. Theoretically speaking, this process of empirical representation might be applied with any degree of accuracy; we might include still higher powers in the formula, and with sufficient labour obtain the values of the constants, by using an equal number of experimental results.

In a similar manner all periodic variations may be represented with any required degree of accuracy by formulæ involving the sines and cosines of angles and their multiples. The form of any tidal or other wave may thus be expressed, as Sir G. B. Airy has explained[e]. Almost all the phenomena registered by meteorologists are periodic in character, and when freed from disturbing causes may be embodied in empirical formulæ. Bessel has given a rule by which from any regular series of observations we may, on the principle of the method of least squares, calculate out with a moderate amount of labour a formula expressing the variation of the quantity observed, in the most probable manner. In meteorology three or four terms are usually sufficient for representing any periodic phenomenon, but the calculation might be carried to any higher degree of accuracy. As the details of the process have been described by Sir John Herschel in his admirable treatise on Meteorology[f], I need not further enter into them.

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[c] 'Chemical Reports and Memoirs,' Cavendish Society, p. 294.

[d] Jamin, 'Cours de Physique,' vol. ii. p. 38.

[e] 'On Tides and Waves,' Encyclopædia Metropolitana, p. 366*.

The reader might be tempted to think that in these processes of calculation we have an infallible method of discovering inductive laws, and that my previous statements (Chap. VII.) as to the purely tentative and inverse character of the inductive process are negatived. Were there indeed any general method of inferring laws from facts it would overturn my statement, but it must be carefully observed that these empirical formulæ do not coincide with natural laws. They are only approximations to the results of natural laws founded upon the general principles of approximation. It has already been pointed out that however complicated be the nature of a curve we may examine so small a portion of it, or we may examine it with such rude means of measurement, that its divergence from an elliptic curve will not be apparent. As a still ruder approximation a portion of a straight line will always serve our purpose ; but if we need higher precision a curve of the third or fourth degree will almost certainly be sufficient. Now empirical formulæ really represent these approximate curves, but they give us no information as to the precise nature of the curve itself to which we are approximating. In another mode of expression we may say that we do not learn what function the variant is of the variable, but we obtain another function which, within the bounds of our observation, gives nearly the same series of values.

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[f] 'Encyclopædia Britannica,' art. Meteorology. Reprint §§ 152–156.

Discovery of Rational Formulæ.

Let us now proceed to consider the modes in which from numerical results we can establish the actual relation between the quantity of the cause and that of the effect. What we want is a *rational* formula or function, which may exhibit the *reason* or exact character and origin of the law in question. There is no word more frequently used by mathematicians than the word *function*, and yet it is difficult to define its meaning with perfect accuracy. Originally it meant performance or execution, being equivalent to the Greek λειτουργία or τέλεσμα. Mathematicians at first used it to mean *any power of a quantity*, but afterwards generalized it so as to include 'any quantity formed in any manner whatsoever from another quantity[g].' Any quantity, then, which depends upon and varies with another quantity may be called a function of it, and either may be considered a function of the other.

Given the quantities, we want the function of which they are the values. It may first of all be pointed out that simple inspection of the numbers cannot as a general rule disclose the function. In an earlier part of this work (vol. i. p. 142) I put before the reader certain numbers, and requested him to point out the law which they obey, and the same question will have to be asked in every case of quantitative induction. There are perhaps three methods, more or less distinct, by which we may hope to obtain an answer :

(1) By purely haphazard trial.

(2) By noting the general character of the variation of

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[g] Lagrange, 'Leçons sur le Calcul des Fonctions,' 1806, p. 4.

the quantities, and trying by preference functions which give a similar form of variation.

(3) By deducing from previous knowledge the form of the function which is most likely to suit.

Having certain numerical results we are always at perfect liberty to invent any kind of mathematical formula we like, and then try whether, by the suitable selection of values for the unknown constant quantities we can make it give the required results. If ever we fall upon a formula which does so, to a fair degree of approximation, there is a presumption in favour of its being the true function, although there is no certainty whatever in the matter. In this way I happened to discover a simple mathematical law which closely agreed with the results of certain experiments on muscular exertion. This law was afterwards shown by Professor Haughton to be the true rational law according to his theory of muscular action [h].

But the chance of succeeding in this manner is usually very small. The number of possible functions is certainly infinite, and even the number of comparatively simple functions is so very large that the probability of falling upon the correct one by mere chance is very slight. Let the reader observe that even when we can thus obtain the law it is by a deductive process, not by showing that the numbers give the law, but that the law gives the numbers.

In the second place, we may, by a survey of the numbers, gain a general notion of the kind of law they are likely to obey, and we may be much assisted in this process by drawing them out in the form of a curve, as will be presently considered. We can in this way ascertain with some probability whether the curve is likely to be a closed one, or whether it has infinite branches; whether such branches are asymptotic, that is, approach indefinitely towards straight lines; whether it is logarithmic in character, or trigonometric. This indeed we can only do if we remember the results of previous investigations. The process is still inversely deductive, and consists in noting what laws gave particular curves, and then inferring inversely that such curves belong to such laws. If we can in this way discover the class of functions to which the required law belongs, our chances of complete success are much increased, because our haphazard trials are now reduced within a narrower sphere. But, unless we have almost the whole curve before us, the identification of its character must be a matter of great uncertainty; and if, as in most physical investigations, we have a mere fragment of the curve, the assistance given would be quite illusory. Curves of almost any character can be made to approximate to each other for a limited extent, so that it is only by a kind of *divination* that we can fall upon the actual function, unless we have theoretical knowledge of the kind of function applicable to the case.

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[h] Haughton, 'Principles of Animal Mechanics,' 1873, pp. 444–450. *Nature*, 30th of June, 1870, vol. ii. p. 158.

When we have once obtained what we believe to be the correct form of function, the remainder of the work is mere mathematical computation to be performed infallibly according to fixed rules[i], which include those employed in the determination of empirical formulæ (vol. ii. p. 110). The function will involve two or three or more unknown constants, the values of which we need to determine by our experimental results. Selecting some of our results widely apart and nearly equidistant, we must form by means of them as many equations as there are constant quantities to be determined. The solution of these equations will then give us the constants required, and having now the actual function we can try whether it gives with sufficient accuracy the remainder of our experimental results. If not, we must either make a new selection of results to give a new set of equations, and thus obtain a new set of values for the constants, or we must acknowledge that our form of function has been wrongly chosen. If it appears that the form of function has been correctly ascertained, we may regard the constants as only approximately accurate and may proceed by the Method of Least Squares (vol. i. p. 458) to determine the most probable values as given by the whole of the experimental results.

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[i] See Jamin, 'Cours de Physique,' vol. ii. p. 50.

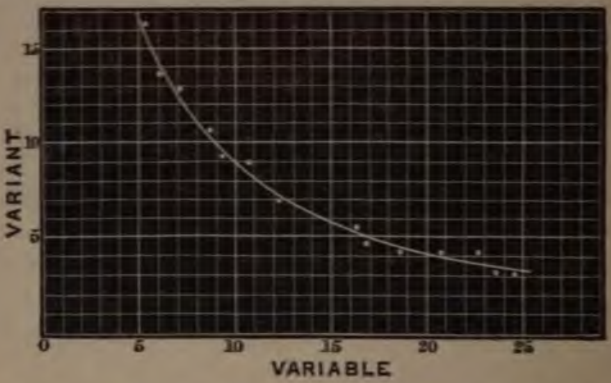
In most cases we shall find ourselves obliged to fall back upon the third mode, that is, anticipation of the form of the law to be expected on the ground of previous knowledge. Theory and analogical reasoning must be our guides. The general nature of the phenomenon will often indicate the kind of law to be looked for. If one form of energy or one kind of substance is being converted into another, we may expect the law of direct simple proportion. In one distinct class of cases the effect already produced influences the amount of the ensuing effect, as for instance in the cooling of a heated body, when the law will be of an exponential form. When the direction in which a force acts influences its action, trigonometrical functions must of course enter. Any force or influence which spreads freely through tridimensional space will be subject to the law of the inverse square of the distance. From such considerations we may sometimes arrive deductively and analogically at the general nature of the mathematical law required.

The Graphical Method.

In endeavouring to discover the mathematical law obeyed by experimental results it is often necessary, and almost always desirable, to call in the aid of space-representations. Every equation involving two variable quantities corresponds to some kind of plane curve, and every plane curve may be represented symbolically in an equation of a more or less complex character, containing two unknown quantities. Now in an experimental research we obtain a number of values of the variant corresponding to an equal number of values of the variable; but all the numbers are affected by more or less error, and the values of the variable will often be irregularly disposed. Even if the numbers were absolutely correct and disposed at regular intervals, there is, as we have seen, no direct mode of discovering the law, but the difficulty of discovery is much increased by the uncertainty and irregularity of the results.

Under such circumstances, the best mode of proceeding is to procure or prepare a paper divided into small equal rectangular spaces, a convenient size for the spaces being one-tenth of an inch square. The values of the variables being marked off along the scale formed by the lowest horizontal line, a point is marked for each corresponding value of the variant perpendicularly above that of the variable, and at such a height as corresponds to the amount of the variant.

The exact scale of the drawing is not of much importance, but it may require to be adjusted according to circumstances, and different values must often be attributed to the upright and horizontal divisions, so as to make the variations conspicuous, but not excessive. If now a curved line be drawn through all the extremities of the ordinates, it will probably exhibit many irregular inflections, owing to the errors which affect all the numbers. But, when the results are numerous, it soon becomes apparent which results are more divergent than others, and guided by a so-called *sense of continuity*, it becomes possible to trace a line among the points which will approximate to the true law more nearly than the points themselves. The accompanying figure sufficiently explains itself.



Perkins employed this graphical method with much care in exhibiting the results of his experiments on the compression of water[k]. The numerical results were marked upon a sheet of paper very exactly ruled at intervals of one-tenth of an inch, and the original marks were left in order that the reader might judge of the correctness of the curve drawn, or choose another for himself. Regnault carried the method to perfection by laying off the points with a small screw dividing engine[l]; and he then formed a table of results by drawing a continuous curve, and measuring its height for equidistant values of the variable.

Not only does a curve drawn in this manner enable us to assign by measurement numerical results more free from accidental errors than any of the numbers obtained directly from experiment, but the form of the curve sometimes indicates the class of functions to which our results belong

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[k] 'Philosophical Transactions,' 1826, p. 544.

[l] Jamin, 'Cours de Physique,' vol. ii. p. 24, &c.

Engraved sheets of paper ready prepared for the drawing of curves may be obtained from Mr. Stanford, at 6 and 7 Charing Cross, or from Messrs. W. and A. K. Johnston, of London and Edinburgh. When we do not require great accuracy, paper ruled by the common machine-ruler into equal squares of about one-fifth or one-sixth of an inch square will serve well enough. I have found Vere Foster's Exercise Book, No. 12[m], which is ruled in this way, very useful for statistical or other numerical purposes. I have also met with engineers' and surveyors' memorandum books ruled with one-twelfth inch squares. When a number of complicated curves have to be drawn, I have found it best to rule a good sheet of drawing paper with lines carefully adjusted at the most convenient distances, and then to prick the points of the curve through it upon another sheet fixed underneath. In this way we obtain an accurate curve upon a blank sheet, and need only introduce such division lines as are requisite to the understanding of the curve.

In some cases our numerical results will correspond, not to the height of single ordinates, but to the area of the curve between two ordinates, or the average height of ordinates between certain limits. If we measure, for instance, the quantities of heat absorbed by water when raised in temperature from 0° to 5°, from 5° to 10°, and so on, these quantities will really be represented by *areas* of the curve denoting the specific heat of water; and, since the specific heat varies continuously between every two points of temperature, we shall not get the correct curve by simply laying off the quantities of heat at the mean temperatures, namely 2½°, 7½°, and so on. Mr. J. W. Strutt has shown that if we have drawn such an incorrect curve, we can with little trouble correct it by a simple geometrical process, and obtain to a very close approximation the true ordinates instead of those denoting areas[n].

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[m] Published by Whittaker & Co., London.

Interpolation and Extrapolation.

When we have by experiment obtained two or more numerical results, and endeavour, without further resort to experiment, to infer and calculate intermediate results, we are said to *interpolate.* If we wish to assign by reasoning results lying beyond the limits of experiment, we may be said, using an expression of Sir George Airy, to *extrapolate.* These two operations are to a certain extent the same in principle, but differ in practicability. It is a matter of great scientific importance to apprehend precisely how far we can interpolate or extend experimental results by extrapolation, and on what grounds we proceed.

In the first place, if the interpolation is to be more than empirical and speculative, we must have not only the experimental results, but the laws which they obey—we must in fact go through the complete process of scientific investigation. Having discovered the laws of nature applying to the case, and verified them by showing that they agree with the experiments in question, we are then in a fair position to anticipate the results of any similar experiments. Our knowledge even now is not certain, because we cannot completely prove the truth of any assumed law, and we cannot possibly exhaust all the circumstances which may more or less affect the result. Even at the best then our interpolations will partake of the want of certainty and precision attaching to all our knowledge of nature. Yet having the supposed laws, our results will be as sure and accurate as any we can attain to. But such a complete procedure is more than we generally mean by interpolation, which generally denotes the employment of some general method of estimating in a merely approximate and probable manner the results which might have been expected independently of any complete theoretical investigation.

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[n] J. W. Strutt, 'On a correction sometimes required in curves professing to represent the connexion between two physical magnitudes.' 'Philosophical Magazine,' 4th Series, vol. xlii. p. 441.

Regarded in this light, interpolation is in reality an indeterminate problem. From given values of a function it is impossible to determine that function; for we can always invent an infinite number of functions which would give those values if we are not restricted by any other conditions, just as through a given series of points we can always draw an infinite number of curves, if we may diverge between or beyond the points into bends and cusps as we think fit[o]. In any process of interpolation we must in fact be guided more or less by *à priori* considerations; we must know, for instance, whether or not periodical fluctuations are to be expected, and we must be guided accordingly in the choice of mathematical formulæ. Supposing, for the present, that the phenomenon is nonperiodic, we next proceed to assume that the function can be expressed in a limited series of the powers of the variable. The number of powers which can be included depends upon the number of experimental results available, and must be at least one less than this number. By processes of calculation, which have been already alluded to in the section on empirical formulæ, we can then calculate the coefficients of the powers, and obtain an empirical formula which will give the required intermediate results. In reality, then, we return to the methods treated under the head of approximation and empirical formulæ; and interpolation, as commonly understood, consists in assuming that a curve of simple character is to pass through certain determined points. If we have, for instance, two experimental results, and only two, we must assume that the curve is a straight line; for the parabolas which can be passed through two points are infinitely various in magnitude, and quite indeterminate. One straight line alone can pass through two points, and it will have an equation of the form $y=mx+n$, the constant quantities of which can be readily determined from two results. Thus, if the two values for x, 7 and 11, give the values for y, 35 and 53, the solution of two simple equations gives $y=4{\cdot}5\times x+3{\cdot}5$ as the equation, and for any other value of x, for instance 10, we get a value of y, 48·5. When we take an exactly intermediate value of x, namely 9, this process yields a simple mean result, namely 44. Three experimental results being given, we may assume that they fall upon a portion of a parabola, and simple algebraic calculation readily gives the position of any intermediate point upon the parabola. Concerning the process of interpolation as practised in the science of meteorology the reader will find some directions in the French edition of Kæmtz' Meteorology[p].

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[o] Herschel, 'Appendix to Translation of Lacroix' Differential Calculus,' p. 551.

When we have, either directly by experiment or by the use of a curve, a series of values of the variant for exactly equidistant values of the variable, it is often very instructive to take the differences between each value of the variant and the next, and then the differences between those differences, and so on. If any series of differences approaches closely to zero it is an indication that the numbers may be correctly represented by a finite empirical formula; if the nth differences are zero, then the formula will contain only the first $n-1$ powers of the variable. Indeed we may sometimes obtain by the Calculus of differences a correct empirical formula; for if p be the first term of the series of values, and Δp, $\Delta^2 p$, $\Delta^3 p$, &c., be the first number in each column of differences, then the mth term of the series of values will be

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[p] 'Cours complet de Météorologie,' traduit par Martins, Note A. du *Traducteur*, p. 449.

$$p + m\Delta p + m\frac{m-1}{2}\Delta^2 p + m\frac{m-1}{2}\frac{m-2}{3}\Delta^3 p + \text{\&c.}$$

A closely equivalent but more practicable formula for interpolation by differences, as devised by Lagrange, will be found in Thomson and Tait's 'Elements of Natural Philosophy,' p. 115.

If no column of differences shows any tendency to become zero throughout, it is an indication that the law is of a more complicated, for instance of an exponential, character, so that it cannot be correctly represented in a formula involving only a few powers of the variable. Dr. J. Hopkinson has lately suggested another method of arithmetical interpolation[q], which is intended to avoid much that is arbitrary in the graphical method. His process will yield the same results in all hands, but he remarks that it has no theoretical basis to rest on.

So far as we can infer the results likely to be obtained by variations beyond the limits of experiment, we must proceed upon the same principles. If possible we must detect the exact laws in action, and then trust to them as a guide when we have no experience. If not, an empirical formula of exactly the same character as those employed in interpolation is our only resource. But the reader must carefully observe that to extend our inference far beyond the limits of experience is exceedingly unsafe. Our knowledge is at the best only approximate, and takes no account of very small tendencies. Now it may, and in fact usually will, happen, that tendencies small within our limits of observation will become perceptible or great under extreme circumstances. When the variable in our empirical formula is small, we are justified in overlooking the existence of higher powers, and taking only two or three of them. But as the variable increases, those higher powers gain in importance, and in time will yield the principal part of the value of the function.

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[q] 'On the Calculation of Empirical Formulæ.' 'The Messenger of Mathematics,' New Series, No. 17, 1872.

This is no mere theoretical inference. Excepting the few great primary laws of nature, such as the law of gravity, the conservation of energy, &c., there is hardly any natural law which we can trust in circumstances widely different from those with which we are practically acquainted. From the expansion or contraction, fusion or vaporisation of substances by heat at the surface of the earth, we can form a most imperfect notion of what would happen near the centre of the earth, where the pressure must almost infinitely exceed anything possible in our experiments. The physics of the earth again give us a feeble, and probably often a misleading, notion of a body like the sun, in most parts of which an almost inconceivably high temperature is united with an inconceivably high pressure. If, as is probable, there are in the realms of space many nebulæ consisting of incandescent and unoxydized vapours of metals and other elements, so highly heated perhaps that chemical composition is out of the question, we are hardly able to treat them as subjects of scientific inference. Hence arises the great importance of any experiments in which we can investigate the properties of substances under extreme circumstances of cold or heat, density or rarity, intense electric excitation, &c. It should be observed that this insecurity in extending our inferences wholly arises from the purely approximate character of our measurements. Had we the power of appreciating indefinitely small quantities, we should by the principle of continuity discover some trace of every change which a substance could undergo under unattainable circumstances. By observing, for instance, the tension of aqueous vapour between 0° and 100° C., we ought theoretically to be able to infer its tension at every other temperature; but this is out of the question because we cannot really ascertain the law precisely between those temperatures.

Many instances might be given to show that laws which appear to represent correctly the results of experiments within certain limits altogether fail beyond those limits. The experiments of Roscoe and Dittmar, on the absorption of gases in water[r] afford many interesting illustrations, especially in the case of hydrochloric acid, the quantity of which dissolved in water under different pressures follows very closely a linear law of variation, from which however it diverges very widely at low pressures[s]. Sir J. Herschel having deduced from various recorded observations of the double star γ Virginis, an elliptic orbit for the motion of one component round the centre of gravity of both, found that for a certain time the motion of the star agreed very well with this orbit. Nevertheless a divergence began to appear by degrees, and after a time became so great that an entirely new orbit, of more than double the linear dimensions of the old one, had ultimately to be adopted[t].

Illustrations of Empirical Quantitative Laws.

Although our chief object in every quantitative inquiry must be to discover the exact or rational formulæ, expressing the general laws of nature applying to the subject, it is instructive to observe in how many important branches of science, no precise laws have yet been detected. The tension of aqueous vapour at different temperatures has been determined by a succession of eminent experimentalists, Dalton, Kæmtz, Dulong, Arago, Magnus, and Regnault, and by the last mentioned the measurements were conducted with all accuracy apparently attainable at present. Yet no incontestible general law has been established. Several functions have been proposed to express the elastic force of the vapour as depending on the temperature. The first general form is that of Young, namely $F = (a + b\,t)^m$, in which a, b and m are unknown quantities to be determined by comparison with observation. Roche has proposed, on theoretical grounds, a complicated formula of an exponential form, and a third form of function is that of Biot, as follows—$\log F = a + b\alpha^t + c\,\beta^t$[u]. I mention these formulæ particularly, because they well illustrate the feeble powers of empirical inquiry. None of the formulæ can be made to correspond exactly with experimental results, and the last two forms correspond nearly equally well. But there is very little probability that the real law has been reached, and it is highly unlikely that it will be discovered except by deduction from mechanical theory.

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[r] Watts's 'Dictionary of Chemistry,' vol. ii. p. 790.

[s] 'Quarterly Journal of the Chemical Society,' vol. viii. p. 15.

[t] 'Results of Observations at the Cape of Good Hope,' p. 293.

The same remarks may be made upon any other laws except those of the most simple character. A vast amount of the most ingenious labour has been spent upon the discovery of some general law of atmospheric refraction. Tycho Brahe and Kepler commenced the inquiry: Cassini first formed a table of refractions, calculated on theoretical grounds: Newton entered into some profound investigations upon the subject: Brooke Taylor, Bouguer, Simpson, Bradley, Mayer, and Kramp successively attacked the question, which is of the highest practical importance as regards the correction of astronomical observations. Laplace next laboured on the subject without exhausting it, and Brinkley and Ivory have since treated it. A closely connected problem, that regarding the relation between the pressure and elevation in different strata of the atmosphere, has received the attention of a long succession of physicists and was most carefully investigated by Laplace. Yet no invariable and general law has been detected. The same may be said concerning the law of human mortality; abundant statistics on this subject are available, and many hypotheses more or less satisfactory have been put forward as to the general form of the curve of mortality, but it seems to be impossible to discover more than an approximate law.

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[u] Jamin, 'Cours de Physique,' vol. ii. p. 138.

It may perhaps be urged that in such subjects no single invariable law can be expected. The atmosphere may be divided into several variable strata which by their unconnected changes frustrate the exact calculations of astronomers. Human life may be subject at different ages to a succession of different influences incapable of reduction under any one law. The results observed may in fact be aggregates of an immense number of separate results each governed by their own separate laws, so that the subjects may be complicated beyond the possibility of complete resolution by empirical methods. This is certainly true of the mathematical functions which must some time or other be introduced into the science of political economy.

Simple Proportional Variation.

When we first treat numerical results in any novel kind of investigation, our impression will probably be that one quantity varies in *simple proportion* to another, so as to obey the law $y=mx+n$. We must learn to distinguish carefully between the cases where this proportionality is really, and where it is only apparently true. When considering the principles of approximation we found that a small portion of any curve will appear to be a straight line. Whenever our modes of measurement are comparatively rude, we must expect to be unable to detect the curvature. Thus Kepler made meritorious attempts to discover the law of refraction, and he slightly approximated to it when he observed that the angles of incidence and refraction *if small* bear a constant ratio to each other. Angles when small are very nearly as their sines, so that he reached an approximate result of the true law. Cardan assumed, probably as a mere guess, that the force required to sustain a body on an inclined plane was simply proportional to the angle of elevation of the plane. This is approximately the case when the angle is very small, and it becomes true again when the angle is a right angle; but in reality the law is much more complicated, the power required being proportional to the sine of the angle. The early thermometer-makers were quite unaware whether the expansion of mercury was exactly proportional or not to the heat communicated to it, and it is only in the present century that we have learnt it to be not so. We now know that even gases obey the law of uniform expansion by heat only in an approximate manner. Until some reason to the contrary is shown, we should do well to look upon every law of simple proportion as only provisionally true.

Nevertheless, there are many of the most important laws of nature which are in the form of simple proportions. Wherever a uniform cause acts in independence of its previous effects, we may expect this relation. Thus, an accelerating force acts equally upon a moving and a motionless body. Hence the velocity produced is always in simple proportion to the force, and also to the duration of its uniform action. As gravitating bodies never interfere with each other's gravity, this force is in direct simple proportion to the mass of each of the attracting bodies, the mass being measured by, or proportional to inertia. Similarly, in all cases of 'direct unimpeded action,' as Sir J. Herschel has remarked[x], we may expect simple proportion to manifest itself. In such cases the equation expressing the relation may have the still simpler form $y=mx$.

A similar simple relation holds true wherever there is a conversion of one substance or form of energy into another. The quantity of chloride of silver is proportional to the quantity either of chlorine or silver. The amount of heat produced in friction is exactly proportional to the mechanical energy absorbed. It was experimentally proved by Faraday that 'the chemical power of the current of electricity is in direct proportion to the quantity of electricity which passes.' When an electric current is produced, the quantity of electric energy is simply proportional to the weight of metal dissolved. If electricity is turned into heat, there is again simple proportion. Wherever, in fact, one thing is but another thing with a new aspect, we may expect to find the law of simple proportion. It is only among the most elementary causes and effects that this simple relation will hold true. Simple conditions do not, generally speaking, produce simple results. The planets move in approximate circles round the sun, but the apparent motions, as seen from the earth, are so various, that men have not believed in such a simple view of the matter for more than about two centuries and a half. All those motions, again, are summed up in the law of gravity, of no great complexity, yet men never have, and never can be, able to exhaust the complications of action and reaction, even among a small number of planets. We should be on our guard against a tendency to assume that the connexion of cause and effect is one of direct proportion. Bacon reminds us of the woman in Æsop's fable, who expected that her hen, with a double measure of barley, would lay two eggs a day instead of one, whereas it thereby grew fat, and ceased to lay any eggs at all.

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[x] 'Preliminary Discourse,' &c. p. 152.

CHAPTER XXIII.

THE USE OF HYPOTHESIS.

IF the views of induction upheld in this work be correct, all inductive investigation consists in a marriage of hypothesis and experiment. When facts are already in our possession, we frame an hypothesis to explain their mutual relations, and by the success or non-success of this explanation is the value of the hypothesis to be entirely judged. In the framing and deductive treatment of such hypotheses, we must avail ourselves of the whole body of scientific truth already accumulated, and when once we have obtained a probable hypothesis, we must not rest until we have verified it by comparison with new facts. By deductive reasoning and calculation, we must endeavour to anticipate such new phenomena, especially those of a singular and exceptional nature, as would necessarily happen if the hypothesis be true. Out of the infinite number of observations and experiments which are possible at every moment, theory must lead us to select those few critical ones which are suitable for confirming or negativing our anticipations.

This work of inductive investigation cannot be guided by any system of precise and infallible rules, like those of deductive reasoning. There is, in fact, nothing to which we can apply rules of method, because the laws of nature to be treated must be in our possession before we can treat them. If, indeed, there were any single rule of inductive method, it would direct us to make an exhaustive arrangement of facts in all possible orders. Given a certain number of specimens in a museum, we might arrive at the best possible classification by going systematically through all possible classifications, and, were we endowed with infinite time and patience, this would be an effective method. It doubtless is the method by which the first few simple steps are taken in every incipient branch of science. Before the dignified name of science is applicable, some coincidences will chance to force themselves upon the attention. Before there was a science of meteorology, or any comprehension of the true conditions of the atmosphere, all observant persons learned to associate a peculiar clearness of the atmosphere with coming rain, and a colourless sunset with fine weather. Knowledge of this kind is called *empirical*, as seeming to come directly from experience; and there is doubtless a considerable portion of our knowledge which must always bear this character.

We may be obliged to trust to the casual detection of coincidences in those branches of knowledge where we are deprived of the aid of any guiding notions; but a very little reflection will show the utter insufficiency of haphazard experiment, when applied to investigations of a complicated nature. At the best, it will be the simple identity, or partial identity, of classes, as illustrated in pp. 146–154 of the first volume, which can be thus detected. It was pointed out that, even when a law of nature involves only two circumstances, and there are one hundred distinct circumstances which may possibly be connected, there will be no less than 4950 pairs of circumstances between which a coincidence may exist. When a law involves three or more circumstances, the possible number of coincidences becomes *vastly* greater still. When considering, again, the subject of combinations and permutations, it became apparent that we could never cope with the possible variety of nature. An exhaustive examination of the metallic alloys, or chemical compounds which can be formed, was found to be out of the question (vol. i. p. 218). It is on such considerations that we can explain the very small additions made to our knowledge by the alchemists. Many of them were men of the greatest acuteness, and their indefatigable labours were pursued through many centuries. A few of the more common compounds and phenomena were discovered by them, but a true insight into the principles of nature, now enables chemists to discover far more useful facts in a single year than were yielded by the alchemists during many centuries. There can be no doubt that Newton was really an alchemist, and often spent his days and nights in laborious experiments. But in trying to discover the secret by which gross metals might be rendered noble, his lofty powers of deductive investigation were wholly useless. Deprived of all guiding clues, his experiments must have been, like those of all the alchemists, purely tentative and haphazard. While his hypothetical and deductive investigations have given us the true system of nature, and opened the way in almost every one of the great branches of natural philosophy, the whole results of his tentative experiments are comprehended in a few happy guesses, given in his celebrated 'Queries.'

Even when we are engaged in apparently passive observation of a phenomenon, which we cannot modify experimentally, it is advantageous that our attention should be guided by some theoretical anticipations. A phenomenon which seems simple is, in all probability, really complex, and unless the mind is actively engaged in looking for particular details, it is quite likely that the most critical circumstances will be passed over. Bessel regretted that no distinct theory of the constitution of comets had guided his observations of Halley's comet[a]; in attempting to verify or refute any good hypothesis, not only would there have been a chance of establishing a true theory, but if confuted, the very confutation would probably have involved a large store of useful observations.

It would be an interesting work, but one which I cannot undertake, to trace out the gradual reaction which has taken place in recent times against the purely empirical, or Baconian, theory of induction. Francis Bacon, seeing the futility of the scholastic logic, which had long been predominant, asserted that the accumulation of facts and the careful and orderly abstraction of axioms, or general laws from them, constituted the true method of induction. This method, as far as we can gather its exact nature from Bacon's writings, would correspond to the process of exhaustive examination and classification to which I have just alluded. The value of this method might be estimated historically by the fact that it has not been followed by any of the great masters of science. Whether we look to Galileo, who preceded Bacon, to Gilbert, his contemporary, or to Newton and Descartes, his successors, we find that discovery was achieved by the exactly opposite method to that advocated by Bacon. Throughout Newton's works, as I shall more fully show in succeeding pages, we find deductive reasoning wholly predominant, and experiments are employed, as they should be, to confirm or refute hypothetical anticipations of nature. In my 'Elementary Lessons in Logic' (p. 258), I stated my belief that there was no kind of reference to Bacon in Newton's works. I have since found that Newton does once or twice employ the expression *experimentum crucis* in his 'Opticks,' but this is the only expression, so far as I am aware, which could indicate on the part of Newton direct or indirect acquaintance with Bacon's writings[b].

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[a] Tyndall, 'On Cometary Theory,' Philosophical Magazine, April, *1869. 4th* Series, vol. xxxvii. p. 243.

Other great physicists of the same age were equally prone to the use of hypothèses rather than the blind accumulation of facts in the Baconian manner. Hooke emphatically asserts in his posthumous work on Philosophical Method, that the first requisite of the Natural Philosopher is readiness at guessing the solution of many phenomena and making queries. 'He ought to be very well skilled in those several kinds of philosophy already known, to understand their several hypotheses, suppositions, collections, observations, &c., their various ways of ratiocinations and proceedings, the several failings and defects, both in their way of raising, and in their way of managing their several theories: for by this means the mind will be somewhat more ready at guessing at the solution of many phenomena almost at first sight, and thereby be much more prompt at making queries, and at tracing the subtlety of Nature, and in discovering and searching into the true reason of things.'

We find Horrocks, again, than whom no one was more filled with the scientific spirit, telling us how he tried theory after theory in order to discover one which was in accordance with the motions of Mars[c]. It might readily be shown again that Huyghens, who possessed one of the most perfect philosophical intellects, followed the deductive process combined with continual appeal to experiment, with a skill closely analogous to that of Newton. As to Descartes and Leibnitz, their investigations were too much opposed to the Baconian rules, since they too often adopted hypothetical reasoning to the exclusion of experimental verification. Throughout the eighteenth century science was supposed to be advancing by the pursuance of the Baconian method, but in reality hypothetical investigation was the main instrument of progress. It is only in the present century that physicists began to recognise this truth. So much opprobrium had been attached by Bacon to the use of hypotheses, that we find Young speaking of them in an apologetic tone. 'The practice of advancing general principles and applying them to particular instances is so far from being fatal to truth in all sciences, that when those principles are advanced on sufficient grounds, it constitutes the essence of true philosophy[d]'; and he quotes cases in which Sir Humphry Davy trusted to his theories rather than his experiments. The late Sir John Herschel, who was both a practical physicist and an abstract logician, always entertained the deepest respect for Bacon, and made the 'Novum Organum' as far as possible the basis of his admirable 'Discourse on the Study of Natural Philosophy.' Yet we find him in Chapter VII fully recognising the part which the formation and verification of theories forms in the higher and more general investigations of physical science. The late Mr. J. S. Mill carried on the reaction by recognising as a distinct method the Deductive Method in which Ratiocination, that is, deductive reasoning, is employed for the discovery of new opportunities of testing and verifying a hypothesis. His main error consisted in the fact that throughout the other parts of his system he inveighed against the value of the deductive process, and even asserted from time to time that every process of reasoning is inductive. In fact Mill fell into much confusion in the use of the words induction and deduction, because he failed to observe that the inverse use of deduction constitutes induction.

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[b] See 'Philosophical Transactions,' abridged by Lowthorp. 4th edit. vol. i. p. 130.

[c] Horrocks, 'Opera Posthuma' (1673), p. 276.

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[d] Young's Works, vol. i. p. 593.

Even Francis Bacon was not wholly unaware of the value of hypothetical anticipation. In one or two places he incidentally acknowledges it, as when he remarks that the subtlety of nature surpasses that of reason, adding that 'axioms abstracted from particular facts in a careful and orderly manner, readily suggest and mark out new particulars.'

The true course of inductive procedure is that which has yielded all the more lofty and successful results of science. It consists in *Anticipating Nature,* in the sense of forming hypotheses as to the laws which are probably in operation; and then observing whether the combinations of phenomena are such as would follow from the laws supposed. The investigator begins with facts and ends with them. He uses such facts as are in the first place known to him in suggesting probable hypotheses; deducing other facts which would happen if a particular hypothesis is true, he proceeds to test the truth of his notion by fresh observations or experiments. If any result prove different from what he expects, it leads him either to abandon or to modify his hypothesis; but every new fact may give some new suggestion as to the laws in action. Even if the result in any case agrees with his anticipations, he does not regard it as finally confirmatory of his theory, but proceeds to test the truth of the theory by new deductions and new trials.

The investigator in such a process is assisted by the whole body of science previously accumulated. He may employ analogy, as I shall point out, to guide him in the choice of hypotheses. The manifold connexions between one science and another may give him strong clues to the kind of laws to be expected, and he thus always selects out of the infinite number of possible hypotheses those which are, as far as can be foreseen at the moment, most probable. Each experiment, therefore, which he performs is that most likely to throw light upon his subject, and even if it frustrate his first views, it probably tends to put him in possession of the correct clue.

Requisites of a Good Hypothesis.

There will be no difficulty in pointing out to what conditions, or rather to what condition an hypothesis must conform in order to be accepted as valid and probable. That condition, as I conceive, is the single one of enabling us to infer the existence of phenomena which occur in our experience. *Agreement with fact is the one sole and sufficient test of a true hypothesis.*

Hobbes, indeed, has named two conditions which he considers requisite in an hypothesis, namely, (1) That it should be conceivable and not absurd; (2) That it should allow of phenomena being necessarily inferred. Boyle, in noticing Hobbes' views, proposed to add a third condition, to the effect that the hypothesis should not be inconsistent with any other truth or phenomenon of nature[e]. Of these three conditions, I am inclined to think that the first cannot be accepted, unless by *inconceivable* and *absurd* we mean self-contradictory or inconsistent with the laws of thought and nature. I shall have to point out that some of the most sure and satisfactory theories involve suppositions which are wholly *inconceivable* in a certain sense of the word, because the mind cannot sufficiently extend its ideas to frame a notion of the actions supposed to exist. That the force of gravity should act instantaneously between the most distant parts of the planetary system, or that a ray of violet light should consist of about 700 billions of vibrations in each second, are statements of an inconceivable and absurd character in one sense; but they are so far from being opposed to fact that we cannot on any other suppositions account for the phenomena observed. But if an hypothesis involve self-contradiction, or is inconsistent with known laws of nature, it is so far self-condemned. We cannot even apply processes of deductive reasoning to a self-contradictory notion; and being entirely opposed to the most general and certain laws known to us, the primary laws of thought, it thereby conspicuously fails to agree with facts. Since nature, again, is never self-contradictory, we cannot at the same time accept two theories which lead to contradictory results. If the one agrees with nature, the other cannot. Hence if there be a law which we believe with high probability to be verified in observation, we must not frame an hypothesis in conflict with it, otherwise the hypothesis will necessarily be in disagreement with observation. Since no law or hypothesis is proved, indeed, with absolute certainty, there is always a chance, however slight, that the new hypothesis may displace the old one; but the greater the probability which we assign to that old hypothesis, the greater must be the evidence required in favour of the new and conflicting one. A decisive *experimentum crucis* to negative the one, and establish the other, will probably be requisite to allay the strife.

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[e] Boyle's 'Physical Examen,' p. 84.

I am inclined to assert, then, that there is but one test of a good hypothesis, namely, *its conformity with observed facts;* but this condition may be said to involve, at the same time, three minor conditions, nearly equivalent to those suggested by Hobbes and Boyle, namely:—

(1) That it allow of the application of deductive reasoning and the inference of consequences.

(2) That it do not conflict with any laws of nature, or of mind, which we hold as true.

(3) That the consequences inferred do agree with facts of observation.

The First Requisite—Possibility of Deductive Reasoning.

As the truth of an hypothesis is to be proved by its conformity with fact, the first condition is that we be able to apply methods of deductive reasoning, and learn what should happen according to such an hypothesis. Even if we could imagine an object acting according to laws wholly unknown in other parts of nature, it would be useless to do so, because we could never decide whether it existed or not. We can only infer what would happen under supposed conditions by applying what knowledge we possess of nature to those conditions. Hence, as Boscovich truly said, we are to understand by hypotheses 'not fictions altogether arbitrary, but suppositions conformable to experience or analogy.' It follows that every hypothesis worthy of consideration must suggest some likeness, analogy, or common law, acting in two or more things. If, in order to explain certain facts, a, a', a'', &c., we invent a cause A, then we must in some degree appeal to experience as to the mode in which A will act. As the objects and laws of nature are certainly not known to the mind intuitively, we must point out some other cause B, which supplies the requisite notions, and all we do is to invent a fourth term to an analogy. As B is to its effects b, b', b'', &c., so is A to its effects a, a', a'', &c. When, for instance, we attempt to explain the passage of light and heat radiations through space unoccupied by matter, we imagine the existence of the so-called *ether*. But if this ether were wholly different from anything else known to us, we should in vain try to reason about it. We must *at least* apply to it the laws of motion, that is, we must so far liken it to matter. And as when applying those laws to the elastic medium air, we are able to infer the phenomena of sound, so by arguing in a similar manner concerning ether we are able to infer the existence of light phenomena corresponding to what do occur. All that we do is to take a material elastic substance, increase its elasticity in an almost indefinite degree, and denude it of gravity and some others of the ordinary properties of matter, but we must retain sufficient likeness to matter to allow of deductive calculations.

The force of gravity is in some respects an almost incomprehensible existence, but in other respects entirely conformable to experience. We can distinctly observe that the force is proportional to mass, and that it acts in entire independence of the other matter which may be present or intervening. The law of the decrease of intensity as the square of the distance increases, may be observed to hold true of light, sound, and any other influences emanating from a point, and spreading uniformly through space. The law is doubtless connected at this point with the primary properties of space itself, and is so far conformable to our necessary ideas.

It may well be said, however, that no hypothesis can be so much as framed in the mind unless it be more or less conformable to experience. As the material of our ideas is undoubtedly derived from sensation, so we cannot figure to ourselves any existence or agent, but as endowed with some of the properties of matter. All that the mind can do in the creation of new existences is to alter combinations, or by analogy to alter the intensity of sensuous properties. The phenomenon of motion is familiar to sight and touch, and different degrees of rapidity are also familiar: we can pass beyond the limits of sense, and suppose the existence of rapid motion, such as our senses could not measure or observe. We know what is elasticity, and we can therefore in a certain sense figure to ourselves elasticity a thousand or a million times greater than any which is sensuously known to us. The waves of the ocean are many times higher than our own bodies; other waves, we may observe, are many times less; continue the proportion, and we may ultimately arrive at waves as small as those of light. Thus it is that from a sensuous basis the powers of mind enable us to reason concerning agents and phenomena different in an unlimited degree. If no hypothesis then can be absolutely opposed to sense, accordance with experience must always be a question of degree.

In order that an hypothesis may allow of satisfactory comparison with experience, it must possess a certain definiteness, and, generally speaking, a certain mathematical exactness allowing of the precise calculation of results. We must be able to ascertain whether it does or does not agree with facts.

The theory of vortices, on the contrary, did not present any mode of calculating the exact relations between the distances and periods of the planets and satellites; it could not, therefore, undergo that rigorous testing to which Newton scrupulously submitted his theory of gravity before its promulgation. Vagueness and incapability of precise proof or disproof often enables a false theory to live; but with those who love truth, such vagueness should excite the highest suspicion. The upholders of the ancient doctrine of Nature's abhorrence of a vacuum, had been unable to anticipate the important fact that water would not rise more than 33 feet in a common suction pump. Nor when the fact was pointed out could they explain it, except by introducing a special alteration of the theory to the effect that Nature's abhorrence of a vacuum was limited to 33 feet.

The Second Requisite—Consistency with established Laws of Nature.

In the second place an hypothesis must not be contradictory to what we believe to be true concerning Nature. It must not involve self-inconsistency which is opposed to the highest and simplest laws, namely, those of Logic. Neither ought it to be irreconcileable with the simple laws of motion, of gravity, of the conservation of energy, or any parts of physical science which we consider to be established beyond reasonable doubt. Not that we are absolutely forbidden to adopt such an hypothesis, but if we do so we must be prepared to disprove some of the best demonstrated truths in the possession of mankind. The fact that conflict exists means that the consequences of the theory are not verified if previous discoveries are correct, and we must therefore show that previous discoveries are incorrect before we can verify our theory.

An hypothesis will be exceedingly improbable, not to say invalid, if it supposes a substance or agent to act in a manner unknown in other cases; for it then fails to be verified in our knowledge of that substance or agent. Several physicists, especially Euler and Grove, have supposed that we might dispense with any ethereal basis of light, and infer from the interstellar passage of rays that there was some kind of rare gas occupying space. But if so, that gas must be excessively rare, as we may infer from the apparent absence of an atmosphere around the moon, and from many other facts and laws known to us concerning gases and the atmosphere; and yet at the same time it must possess an elastic force at least a billion times as great as atmospheric air at the earth's surface, in order to account for the extreme rapidity of the light rays. Such an hypothesis then is inconsistent with the main body of our knowledge concerning gases.

Provided that there be no clear and absolute conflict with known laws of nature, there is nothing so improbable or apparently inconceivable that it may not be rendered highly probable, or even approximately certain, by a sufficient number of concordances. In fact the two best founded and most conspicuously successful theories in the whole range of physical science involve the most absurd suppositions. Gravity is a force which appears to act between bodies through vacuous space; it is in positive contradiction to the old dictum that nothing could act but through some intervening medium or substance. It is even more puzzling that the force acts in perfect indifference to all intervening obstacles. Light in spite of its extreme velocity, shows much respect to matter, for it is almost instantaneously stopped by opaque substances, and to a considerable extent absorbed and deflected by transparent ones. But to gravity all media are, as it were, absolutely transparent, nay non-existent; and two particles at opposite points of the earth affect each other exactly as if the globe were not between. To complete the apparent impossibility, the action is, so far as we can observe, absolutely instantaneous, so that every particle of the universe is at every moment in separate cognizance, as it were, of the relative position of every other particle throughout the universe at that same moment of absolute time. Compared with such incomprehensible conditions, the theory of vortices deals with common-place realities. Newton's celebrated saying, *hypotheses non fingo*, bears the appearance of pure irony; and it was not without apparent grounds that Leibnitz and the greatest continental philosophers charged Newton with re-introducing occult powers and qualities.

The undulatory theory of light presents almost equal difficulties of conception. We are asked by physical philosophers to give up all our ordinary prepossessions, and believe that the interstellar space which seemed so empty is not empty at all, but filled with *something* immensely more solid and elastic than steel. As Dr. Young himself remarked[f], 'the luminiferous ether, pervading all space, and penetrating almost all substances, is not only highly elastic, but absolutely solid!!!' Sir John Herschel has calculated the amount of force which may be supposed, according to the undulatory theory of light, to be exerted at each point in space, and finds it to be 1,148,000,000,000 times the elastic force of ordinary air at the earth's surface, so that the pressure of the ether upon a square inch of surface must be about 17,000,000,000,000, or seventeen billions of pounds[g]. Yet we live and move without appreciable resistance through this medium, indefinitely harder and more elastic than adamant. All our ordinary notions must be laid aside in contemplating such an hypothesis; yet they are no more than the observed phenomena of light and heat force us to accept. We cannot deny even the strange suggestion of Dr. Young, that there may be independent worlds, some possibly existing in different parts of space, but others perhaps pervading each other unseen and unknown in the same space[h]. For if we are bound to admit the conception of this adamantine firmament, it is equally easy to admit a plurality of such. We see, then, that mere difficulties of conception must not in the least discredit a theory which otherwise agrees with facts, and we must only reject hypotheses which are inconceivable in the sense of breaking distinctly the primary laws of thought and nature.

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[f] Young's 'Works,' vol. i. p. 415.

[g] 'Familiar Lectures on Scientific Subjects,' p. 282.

[h] Young's 'Works,' vol. i. p. 417.

The Third Requisite—Conformity with Facts.

Before we accept a new hypothesis, it must furnish us with distinct credentials, consisting in the deductive anticipation of a series of facts, which are not already connected and accounted for by any equally probable hypothesis. We cannot lay down any precise rule as to the number of accordances which can establish the truth of an hypothesis, because the accordances will vary much in value. While, on the one hand, no finite number of accordances will give entire certainty, the probability of the hypothesis will increase very rapidly with the number of accordances. Seldom, indeed, shall we have a theory free from difficulties and apparent inconsistency with facts. Though one real and undoubted inconsistency would be sufficient to overturn the most plausible theory, yet there is usually some probability that the fact may be misinterpreted, or that some supposed law of nature, on which we are relying, may not be true. Almost every problem in science thus takes the form of a balance of probabilities. It is only when difficulty after difficulty has been successfully explained away, and decisive *experimenta crucis* have, time after time, resulted in favour of our theory, that we can venture to assert the falsity of all objections.

The sole real test of an hypothesis is its accordance with fact. Descartes' celebrated system of vortices is exploded and rejected, not because it was intrinsically absurd and inconceivable, but because it could not give results in accordance with the actual motions of the heavenly bodies. The difficulties of conception involved in the apparatus of vortices, are mere child's play compared with those of gravitation and the undulatory theory already described. The vortices are on the whole plausible suppositions; for the planets and satellites bear at first sight much resemblance to objects carried round in whirlpools, an analogy which doubtless suggested the theory. The failure was in the first and third requisites; for, as already remarked, the theory did not allow of any precise calculation of planetary motions, and was so far incapable of rigorous verification. But so far as we can institute a comparison, facts are entirely against the vortices. Newton carefully pointed out that the Cartesian theory was inconsistent with the laws of Kepler, and would represent the planets as moving more rapidly at their aphelia than at their perihelia[i]. Newton did not ridicule the theory as absurd, but showed[k] that it was 'pressed with many difficulties.' The rotatory motions of the sun and planets on their own axes are in striking conflict with the revolutions of the satellites carried round them; and comets, the most flimsy of bodies, calmly pursue their courses in elliptic paths, altogether irrespective of the vortices which they intersect. We may now also point to the interlacing orbits of the minor planets as a new and insuperable difficulty in the way of the Cartesian ideas.

Newton, though he established the best of theories, was also capable of proposing one of the worst; and if we want an instance of a theory decisively contradicted by facts, we have only to turn to his views concerning the origin of natural colours. Having analysed, with incomparable skill, the origin of the colours of thin plates, he suggests that the colours of all bodies and substances are determined in like manner by the size of their ultimate particles. A thin plate of a definite thickness will reflect a definite colour; hence, if broken up into fragments it will form a powder of the same colour. But, if this be a sufficient explanation of coloured substances, then every coloured fluid ought to reflect the complementary colour of that which it transmits. Colourless transparency arises,

[i] 'Principia,' bk. II. Sect. ix. Prop. 53.

[k] Ibid. bk. III. Prop. 43. General Scholium.

according to Newton, from all the particles being too minute to reflect light; but if so, every transparent substance should appear perfectly black by reflected light, and, *vice versâ*, every black substance should be transparent. Newton himself so acutely felt this last difficulty as to suggest that true blackness is due to some internal refraction of the rays to and fro, and an ultimate stifling of them, which he did not attempt further to explain. Unless some other process came into operation, neither refraction nor reflection, however often repeated, would destroy the energy of light. The theory gives no account, therefore, as Brewster shows, of 24 parts out of 25 of the light which falls upon a black coal, and the $\frac{1}{25}$th part which is reflected from the lustrous surface is equally inconsistent with the theory, because fine coal-dust is almost entirely devoid of reflective power[1]. It is now generally believed that the colours of natural bodies are due to the unequal absorption of rays of light of different refrangibility.

Experimentum Crucis.

As we deduce more and more conclusions from a theory, and find them verified by trial, the probability of the theory increases in a most rapid manner; but we never escape the risk of error altogether. Absolute certainty is beyond the power of inductive investigation, and the most plausible suppositions may ultimately be proved false. Such is the groundwork of similarity in nature, that two very different conditions may often give closely similar results. We sometimes find ourselves therefore in possession of two or more hypotheses which both agree with so many experimental facts as to have great appearance of truth. Under such circumstances we have need of some new experiment, which shall give results agreeing with one hypothesis but not with the other.

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[1] Brewster's 'Life of Newton,' 1st edit. chap. vii.

Any such experiment which decides between two rival theories may be called an *Experimentum Crucis*, an Experiment of the Finger Post. Whenever the mind stands, as it were, at cross-roads, and knows not which way to select, it needs some decisive guide, and Bacon therefore assigned great importance and authority to instances or facts which serve in this capacity. The name given by Bacon has become exceedingly familiar; it is perhaps almost the only one of Bacon's figurative expressions which has passed into common use. We even find Newton, as I have already mentioned, using the name (vol. ii. p. 134).

I do not think, indeed, that the common use of the word at all agrees with that intended by Bacon. Sir John Herschel says that 'we make an experiment of the crucial kind when we form combinations, and put in action causes from which some particular one shall be deliberately excluded, and some other purposely admitted[m].' This, however, seems to be the description of any special experiment not made at haphazard. Pascal's experiment of causing a barometer to be carried to the top of the Puy-de-Dôme has often been considered as a perfect *experimentum crucis*, if not the first distinct one on record[n]; but if so, we must dignify the doctrine of Nature's abhorrence of a vacuum with the position of a rival theory. A crucial experiment must not simply confirm one theory, but must negative another; it must decide a mind which is in equilibrium, as Bacon says[o], between two equally plausible views. 'When in search of any nature, the understanding comes to an equilibrium, as it were, or stands suspended as to which of two or more natures the cause of nature inquired after should be attributed or assigned, by reason of the frequent and common occurrence of several natures, then these Crucial Instances show the true and inviolable association of one of these natures to the nature sought, and the uncertain and separable alliance of the other, whereby the question is decided, the former nature admitted for the cause, and the other rejected. These instances, therefore, afford great light, and have a kind of overruling authority, so that the course of interpretation will sometimes terminate in them, or be finished by them.'

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[m] 'Discourse on the Study of Natural Philosophy,' p. 151.

[n] Ibid. p. 229.

[o] 'Novum Organum,' bk. II. Aphorism 36.

The long continued strife between the Corpuscular and Undulatory theories of light forms the best possible illustration of the need of an Experimentum Crucis. It is highly remarkable in how complete and plausible a manner both these theories agreed with the ordinary laws of geometrical optics, relating to reflection and refraction.

A moving particle, according to the first law of motion, proceeds in a perfectly straight line, when undisturbed by extraneous forces. If the particle, being perfectly elastic, strike a perfectly elastic plane, it will bound off in such a path that the angles of incidence and reflection will be equal. Now a ray of light proceeds in a perfectly straight line, or appears to do so, until it meets a reflecting body, when its path is altered in a manner exactly similar to that of the elastic particle. Here is a remarkable correspondence which probably suggested to Newton's mind that light consisted of minute elastic particles moving with excessive rapidity in straight lines. The correspondence was found to extend also to the law of simple refraction; for if these particles of light be supposed capable of attracting matter, and being attracted by it at insensibly small distances, then a ray of light, falling on the surface of a transparent medium, will suffer an increase in its velocity of motion perpendicular to the surface, and the familiar law of sines is the necessary consequence. This remarkable explanation of the law of refraction had doubtless a very strong effect in leading Newton to entertain the corpuscular theory, and he appears to have thought that the analogy between the propagation of the rays of light and the motion of bodies was perfectly exact, whatever might be the actual nature of light[p]. It is highly remarkable, again, that Newton was able to give, by his corpuscular theory, a plausible explanation of the inflection of light as discovered by Grimaldi. The theory would indeed have been a very probable one could Newton's own law of gravity have been applied; but this was excluded, because the particles of light, in order that they may move in straight lines, must be assumed devoid of any influence upon each other.

The Huyghenian or Undulatory theory of light was also able to explain the same phenomena, but with one remarkable difference. If the undulatory theory be true, light must move more slowly in a dense refracting medium than in a rarer one; but the Newtonian theory assumed that the attraction of the dense medium caused the particles of light to move more rapidly than in the rare medium. On this point, then, there was a complete discrepancy between the two theories, and observation was required to show which theory was to be preferred. Now by simply cutting a uniform plate of glass into two pieces, and slightly inclining one piece so as to increase the length of the path of a ray passing through it, experimenters have been able to show that the light does move more slowly in glass than in air[q]. More recently, in 1850, Fizeau and Foucault independently measured the velocity of light in air and water by a revolving mirror, and found that the velocity is greater in air[r]. There are indeed a number of other points at which experience decides against Newton, and in favour of Huyghens and Young. Euler rejected the Corpuscular theory because particles of matter moving with the immense velocity of light must possess great momentum, of which there is no evidence in fact[s]. Bennet concentrated the light and heat of the sun upon a body so delicately suspended that an exceedingly small amount of momentum must have been rendered apparent, but there was no such effect[t]. This experiment, indeed, is of a negative kind, and is not absolutely conclusive, unless we could estimate the momentum which Newton's theory would require to be present (see vol. ii. p. 45); but there are other difficulties. Laplace pointed out that the attraction supposed to exist between matter and the corpuscular particles of light, would cause the velocity of light to vary with the size of the emitting body, so that if a star were 250 times as great in diameter as our sun, its attraction would prevent the emanation of light altogether[u]. But so far as experience shows, the velocity of light is uniform, and independent of the magnitude of the emitting body, as it should be according to the undulatory theory. Lastly, Newton's explanation of diffraction or inflection fringes of colours was only *plausible*, and not true; for Fresnel ascertained that the dimensions of the fringes are not what they would be according to Newton's theory.

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[p] 'Principia,' bk. I. Sect. xiv. Prop. 96. Scholium, 'Opticks,' Prop. VI. 3rd edit. p. 70.

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[q] Airy's 'Mathematical Tracts,' 3rd edit. pp. 286–288.

[r] Jamin, 'Cours de Physique,' vol. iii. p. 372.

[s] Euler's 'Letters,' vol. ii. Letter XIX. p. 69.

[t] Balfour Stewart, 'Elementary Treatise on Heat,' p. 161.

[u] Young's 'Lectures on Natural Philosophy' (1845), vol. i. p. 361.

Although the Science of Light presents us with the most beautiful examples of crucial experiments and observations, instances are not wanting in other branches of science. Copernicus asserted in opposition to the ancient Ptolemaic theory that the earth and planets moved round the sun, and he predicted that if ever the sense of sight could be rendered sufficiently acute and powerful, we should see phases in Mercury and Venus. Galileo with his telescope was able, in 1610, to verify the prediction as regards Venus, and subsequent observations of Mercury lead to a like conclusion. The discovery of the aberration of light added a new proof, still further strengthened by the more recent determination of the parallax of fixed stars. Hooke proposed to prove the existence of the earth's diurnal motion by observing the deviation of a falling body, an experiment successfully accomplished by Benzenberg; and Foucault's pendulum has since furnished an additional indication of the same motion, which is indeed also apparent in the direction of the trade winds. All these are crucial facts in favour of the Copernican theory.

Davy's discovery of potassium and sodium in 1807 was a good instance of a crucial experiment; for it decisively confirmed Lavoisier's views, and at the same time negatived the ancient notions of phlogiston.

Descriptive Hypotheses.

There are some, or probably many, hypotheses which we may call *descriptive hypotheses*, and which serve for little else than to furnish convenient names. When a certain phenomenon is of an unusual and mysterious kind, we cannot even speak of it without using some analogy. Every word implies some resemblance between the thing to which it is applied, and some other thing, which fixes the meaning of the word. Thus if we are to speak of what constitutes electricity, we must search for the nearest analogy, and as electricity is chiefly characterised by the rapidity and facility of its movements, the notion of a fluid of a very subtle character presented itself as most appropriate. There is the single fluid and the double fluid theory of electricity, and a great deal of discussion has been uselessly spent upon them. The fact is that if these theories be understood as more than convenient modes of describing the phenomena, they are grossly invalid. The analogy extends only to the rapidity of motion, and the fact that a phenomenon occurs successively at different points of the body. The so-called electric fluid adds nothing to the weight of the conductor, and to suppose that it really consists of particles of matter would be even more absurd than to reinstate the Corpuscular theory of light. An infinitely closer analogy exists between electricity and light undulations, which are about equally rapid in propagation; and while we shall probably continue for a long time to talk of the electric fluid, there can be no doubt that this expression merely represents some phase of molecular motion, some wave of disturbance propagating itself at one time through material conductors, at another time through the ethereal basis of light. The invalidity of these fluid theories is moreover shown in the fact that they have not led to the invention of a single new experiment. When we speak of heat as *flowing* from one body to another, we likewise use a descriptive hypothesis merely; for Lambert's theory of the fluid motion of heat is no better than the Corpuscular theory of light.

Among these merely descriptive hypotheses I should be inclined to place Newton's theory of Fits of Easy Reflection and Refraction. That theory has been since *exploded* by actual discordance with fact, but even when really entertained it did not do more than describe what took place. It involved no deep analogy to any other phenomena of nature, for Newton could not point to any other substance which went through these extraordinary changes. We now know that the true analogy would have been the waves of sound, of which Newton had acquired in other respects so complete a comprehension. But though the notion of interference of waves had distinctly occurred to Hooke, Newton had failed to see how the periodic phenomena of light could be connected with the periodic character of waves. His hypothesis fell because it was out of analogy with everything else in nature, and it therefore did not allow him, as in other cases, to descend by mathematical deduction to consequences which could be verified or refuted.

We are always at freedom again to imagine the existence of a new agent or force, and give it an appropriate name, provided there are phenomena incapable of explanation from known causes. We may speak of *vital force* as occasioning life, provided that we do not take it to be more than a name for an undefined something giving rise to inexplicable facts, just as the French chemists called Iodine the Substance X, while they were unaware of its real character and place in chemistry[y]. Encke was quite justified in speaking of the *resisting medium* in space so long as the retardation of his comet could not be otherwise accounted for. But such hypotheses will do much harm whenever they divert us from attempts to reconcile the facts with known laws, or when they lead us to mix up entirely discrete things. We have no right, for instance, to confuse Encke's supposed resisting medium with the ethereal basis of light. The name protoplasm, now so familiarly used by physiologists, is doubtless legitimate so long as we do not mix up different sub-

[y] Paris, 'Life of Davy,' p. 274.

stances under it, or imagine that the name gives us any knowledge of the obscure origin of life. To name a substance protoplasm no more explains the infinite variety of forms of life which spring out of the substance, than does the *vital force* which may be supposed to reside in the protoplasm. Both expressions appear to me to be mere names for an unknown and inexplicable series of causes which out of apparently similar conditions produce the most diverse results.

Hardly to be distinguished from descriptive hypotheses are certain imaginary objects or conditions which we often frame for the more ready investigation or comprehension of a subject. The mathematician, in treating abstract questions of probability, finds it convenient, to represent the conditions to his own or other minds by a concrete analogy in the shape of a material ballot-box. The fundamental principle of the inverse method of probabilities upon which depends the whole of our reasoning in inductive investigations is proved by Poisson, who imagines a number of ballot-boxes, of which the contents are afterwards supposed to be mixed in one great box (vol. i. p. 280). Many other such devices are also used by mathematicians. When Newton investigated the nature of waves, he employed the pendulum as a convenient mode of representing the nature of the undulation. Centres of gravity, oscillation, &c., poles of the magnet, lines of force, are other imaginary existences solely employed to assist our thoughts (vol. i. p. 422). All such creations of the mind may be called *Representative Hypotheses*, and they are only permissible and useful so far as they embody analogies. Their further consideration properly belongs either to the subject of Analogy, or to that of language and representation, founded upon analogy.

CHAPTER XXIV.

EMPIRICAL KNOWLEDGE, EXPLANATION, AND PREDICTION.

THE one great method of inductive investigation, as we have seen, consists in the union of hypothesis and experiment, deductive reasoning being the link by which the experimental results are made to confirm or confute the hypothesis. Now when we consider this relation between hypothesis and experiment, it is obvious that we may classify our knowledge under four heads.

(1) We may be acquainted with facts or phenomena which have come under our notice accidentally or without reference to any special hypothesis, and which have not been brought into accordance as yet with any hypothesis. Such facts constitute what is called *Empirical Knowledge.*

(2) Another very extensive portion of our knowledge consists of those facts which, having been first observed empirically, have afterwards been brought into accordance with other facts by an hypothesis concerning the general laws applying to them. This portion of our knowledge may be said to be *explained, reasoned,* or *generalised.*

(3) In a third place comes the collection of facts, minor in number, but most important as regards their scientific value and interest, which have been anticipated by theory and afterwards verified by experiment.

(4) Lastly, there may and does exist knowledge of phenomena accepted solely on the ground of theory, and which is incapable of experimental confirmation, at least with the instrumental means at the time in our possession.

It is a work of much interest to compare and illustrate in some degree the relative extent and value of these four groups of knowledge. As a general rule we shall observe that every great branch of science originates in facts observed accidentally, or without any distinct consciousness of what is to be expected. But as science progresses, its power of foresight rapidly increases, until the mathematician in his study seems to acquire the power of anticipating nature, and predicting what will happen in stated circumstances before the eye of man has ever witnessed the event.

Empirical Knowledge.

By empirical knowledge we mean such as is derived directly from the examination of certain detached facts, and rests entirely on those facts, without corroboration or connexion with other branches of knowledge. It is contrasted to generalised and theoretical knowledge, which embraces many series of facts under a few simple and comprehensive principles, so that each series serves to throw light upon each other series of facts. Just as, in the map of a half-explored country, we see detached portions of rivers, isolated mountains, and undefined plains, not connected into any general plan, so a new branch of knowledge often consists of groups of facts, each group standing apart, so as not to allow us to reason from one part to another.

Before the time of Descartes, and Newton, and Huyghens, there was much empirical knowledge of the phenomena of light. The rainbow had always struck the attention of the most careless observers, and there was no difficulty in perceiving that its conditions of occurrence consisted in rays of the sun shining upon falling drops of rain. It was impossible to overlook the resemblance of the ordinary rainbow to the comparatively rare lunar rainbow, to the bow which often appears upon the spray of a waterfall, or even upon beads of dew suspended on grass and spiders' webs. In all these cases the uniform conditions are rays of light and round drops of water. Roger Bacon had noticed these conditions, as well as the analogy of the rainbow colours to those produced by crystals[a]. But the knowledge was empirical until Descartes and Newton showed how the phenomena were connected with all the other facts concerning the refraction of light.

There can be no better instance of an empirical truth than that detected by Newton concerning the high refractive powers of combustible substances. Newton's chemical notions were almost as vague as those prevalent in his day, but he observed that certain 'fat, sulphureous, unctuous bodies,' as he calls them, such as camphor, oils, spirit of turpentine, amber, &c., have refractive powers two or three times greater than might be anticipated from their densities[b]. The enormous refractive index of diamond, led him with great sagacity to regard it as also of the same unctuous or inflammable nature, so that he may be regarded as predicting the combustibility of the diamond, afterwards demonstrated by the Florentine Academicians in 1694. Brewster having entered into a long investigation of the refractive powers of different substances, confirmed Newton's assertions, and found that the three elementary combustible substances, diamond, phosphorus, and sulphur, have by far the highest refractive indices known in proportion to their densities[c], and there are only a few substances, such as chromate of lead or glass of antimony, known to exceed them in absolute power of refraction. The oils and hydrocarbons generally possess an excessive index. But this knowledge remains to the present day purely empirical, no connexion having been pointed out between this coincidence of inflammability and high refractive power, with other laws of chemistry or optics. It is worthy of notice, however, as pointed out by Brewster, that if Newton had argued concerning two minerals, Greenockite and Octahedrite, as he did concerning diamond, his predictions would have proved false. In the present day, the relation of the refractive index to the density and atomic weight of a substance is becoming a matter of theory; yet there remain specific differences of refractive power known only on empirical grounds, and it is curious that in hydrogen also an abnormally high refractive power has been found to be joined to inflammability.

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[a] 'Opus Majus.' Edit. 1733. Cap. x. p. 460.

[b] Newton's 'Opticks.' Third edit. p. 249. Leslie's 'Dissertation,' Encyclopaedia Britannica, p. 550.

The science of chemistry, however much its theory may have progressed, still presents us with a vast body of empirical knowledge. Not only is it at present hopeless to attempt to account for the particular group of qualities belonging to each element, but there are multitudes of particular facts of which no further account can be given. Why should the sulphides of many metals be intensely black? Why should a slight amount of phosphoric acid have so great a power of interference with the crystallization of vanadic acid[d]. Why should the compound silicates of alkalies and alkaline metals be transparent? Why should gold be so highly ductile, and gold and silver the only two sensibly translucent metals? Why should sulphur be capable of so many peculiar changes into amorphous conditions?

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[c] Brewster, 'Treatise on New Philosophical Instruments,' p. 266, &c.

[d] *Roscoe*, Bakerian Lecture, 'Philosophical Trans.' (1868), vol. clviii. p. 6.

There are whole branches of chemical knowledge which are as yet mere aggregates of disconnected facts. The properties of alloys, or mixtures of metals, are often exceedingly remarkable; but no laws have yet been detected, and the usual laws of combining proportions seem to have no clear application[e]. Not the slightest explanation can be given of the wonderful variations of the qualities of iron, according as it contains more or less carbon and silicon, nay, even the facts of the case are often involved in uncertainty. Why, again, should the properties of steel be remarkably affected by the presence of a little tungsten. All that was determined by Matthiessen concerning the variation of the conducting powers of copper according to its purity, was of a purely empirical character[f]. Many animal substances cannot be shown to obey even the laws of combining proportions. Thus for the most part chemistry is yet a science occupied with an exact description of artificial or natural substances, which by the collection of enormous numbers of exact facts is preparing the way for an extension of theory at some future time. .

We must not indeed suppose that any science will ever entirely cease to be empirical. Multitudes of phenomena have been explained by the undulatory theory of light; but there remains an almost undiminished mass of facts yet to be treated. The natural colours of bodies, and the rays given off by them when heated, are yet free from all theory, and yield few empirical coincidences. The theory of electricity is partially understood, but the conditions of the production of frictional electricity defy law or explanation, although they have been studied for two centuries or more. I shall subsequently point out that even the establishment of a wide and true law of nature is but the starting-point for the discovery of exceptions or slight divergences giving a wide scope to empirical discovery.

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[e] 'Life of Faraday,' vol. ii. p. 104.

[f] Watts, 'Dictionary of Chemistry,' vol. ii. p. 39, &c.

There is probably no science, I have said, which is entirely free from empirical and unexplained facts. Logic approaches most nearly to this position, as it is merely a deductive development of the laws of thought and the principles of substitution. Yet some of the facts established in the investigation of the inverse logical problem (vol. i. p. 157) may be considered empirical. Mathematical science often yields empirical truths. Why, for instance, should the value of π, when expressed to a great number of figures, contain the digit 7 much less frequently than any other digit[g]? Even geometry may allow of empirical truths, when the matter does not involve quantities of space, but numerical results and the positive or negative character of quantities, as in De Morgan's theorem concerning negative areas.

Accidental Discovery.

There are not a few cases where almost pure accident has undoubtedly determined the moment when a new branch of knowledge was to be created. The true laws of the construction of crystals were not discovered until Haüy happened to drop a beautiful crystal of calc-spar upon a stone pavement. His momentary regret, at destroying a choice specimen, was quickly removed when, in attempting to join the fragments together, he observed regular geometrical faces, which did not correspond with the external facets of the crystals. A great many more crystals were soon broken intentionally, to observe the planes of cleavage, and a nearly complete comprehension of the internal structure of crystalline substances was soon the result. Here we see how much more was due to the reasoning powers of the philosopher, than to an accident which must often have happened to other persons.

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[g] De Morgan's 'Budget of Paradoxes,' p. 291.

In a similar manner, a purely fortuitous occurrence led Malus to discover the polarization of light by reflection. The phenomena of double refraction had, of course, been long known, and when engaged in Paris in 1808, in investigating the character of light thus polarized, Malus chanced to look through a double refracting prism at the light of the setting sun, reflected from the windows of the Luxembourg Palace. In turning the prism round, he was surprised to find that the ordinary image disappeared at two opposite positions of the prism. He remarked that the reflected light behaved exactly like light which had been already polarized by passing through another prism. He was induced to test the character of light reflected under other circumstances, and it was eventually proved that polarization is connected by invariable laws with the act of reflection. Some of the most general laws of optics, previously unsuspected, were thus discovered by pure accident.

In the history of electricity, accident has had a large part. For centuries some of the more common effects of magnetism, or frictional electricity, had presented themselves as exceptional and unaccountable deviations from the ordinary course of Nature. Accident must, of course, have first directed attention to such phenomena, but how few of those who witnessed them had any conception of the all-pervading power thus manifested. The very existence of the so-called galvanism, or electricity of low tension, was unsuspected until Galvani accidentally touched the leg of a frog with pieces of metal. The decomposition of water by voltaic electricity is also said to have been accidentally discovered by Nicholson in 1801, and Davy speaks of this discovery as the foundation of all that had since been done in electro-chemical science.

It is otherwise with the discovery of electro-magnetism, or the relation between the magnet and electricity. Oersted, in common with many others, had suspected the existence of some relation between these strange powers, and he appears to have tried to detect its exact nature. Once, as we are told by Hansteen, he had employed a strong galvanic battery during a lecture, and at the close it occurred to him to try the effect of placing the conducting wire parallel to a magnetic needle, instead of at right angles, as he had previously done. The needle immediately moved and took up a position nearly at right angles to the wire; he inverted the direction of the current, and the needle deviated in a contrary direction. The great discovery was made, and if by accident, it was such an accident as happens only to those who deserve them, as Lagrange remarked of Newton [h]. There was, in fact, nothing accidental, except that, as in all totally new discoveries, Oersted did not know what to look for. He could not infer from previous knowledge the nature of the relation, and it was only repeated trial in different modes which could lead him to the right combination. High and happy powers of inference, and not accident, subsequently induced Faraday to reverse the process, and show that the motion of the magnet would occasion an electric current in the wire.

Sufficient investigation would probably show that almost every branch of art and science had an accidental beginning. In historical times almost every important new instrument, such as the telescope, the microscope, or the compass, was probably suggested by some accidental occurrence or observation. In pre-historic times the germs of the arts must have arisen still more exclusively in the same way. Cultivation of plants probably arose, in Mr. Darwin's opinion, from some such accident as the seeds of a fruit falling upon a heap of refuse, and producing an unusually fine variety. Even the use of fire must, some time or other, have been discovered in a like accidental manner.

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[h] 'Life of Faraday,' vol. ii. p. 396.

With the progress of any branch of science, the element of chance becomes much reduced. Not only are laws discovered which enable results to be predicted, as we shall shortly consider, but the systematic examination of phenomena and substances often leads to important and novel discoveries, which can in no true sense be said to be accidental. It has been asserted that the anæsthetic properties of chloroform were disclosed by a little dog smelling at a saucerful of the liquid in a chemist's shop in Linlithgow, the singular effects upon the dog being reported to Dr. Simpson, who turned the incident to such good account. This story, however, has since been shown to be a fabrication, the fact being that Dr. Simpson had for many years being endeavouring to discover a better anæsthetic than those previously employed, and that he tested the properties of chloroform, among other substances, at the suggestion of Mr. Waldie, a Liverpool chemist. The valuble powers of hydrate of chloral have since been discovered in a like manner, and systematic inquiries are continually being made into the therapeutic or economic value of new chemical compounds.

If we must attempt to draw any conclusion concerning the part which chance plays in scientific discovery, it must be allowed that it more or less affects the success of all inductive investigation, but becomes less important with the progress of any particular branch of science. Accident, too, may bring a new and valuable combination to the notice of some person who had never expressly searched for a discovery of the kind, and the probabilities are certainly in favour of a discovery being occasionally made in this manner. But the greater the tact and industry with which a physicist applies himself to the study of nature, the greater is the probability that he will meet with fortunate accidents, and will turn them to good account. Thus it comes to pass that, in the refined investigations of the present day, genius united to extensive knowledge, cultivated powers and indomitable industry, constitute the characteristics of the great discoverer.

Empirical Observations subsequently Explained.

The second great portion of scientific knowledge consists of facts which have been first learnt in a purely empirical manner, but have afterwards been shown to follow from some law of nature, that is, from some highly probable hypothesis. Facts are said to be explained when they are thus brought into harmony with other facts, or bodies of general knowledge. There are few words more familiarly used in scientific phraseology than this word *explanation*, and it is necessary to decide exactly what we mean by it, since the question touches the very deepest points concerning the nature of science. Like most terms referring to mental actions, the verbs *to explain*, or *to explicate*, involve material similes. The action is *ex plicis plana reddere*, to take out the folds, and render a thing plain or even. Explanation thus renders a thing clearly comprehensible in all its points, so that there is nothing left outstanding or obscure.

Every act of explanation consists in detecting and pointing out a resemblance between facts, or in showing that a greater or less degree of identity exists between apparently diverse phenomena. This resemblance may be of any extent and depth; it may be a general law of nature, which explains and harmonizes the motions of all the heavenly bodies, that is, shows that there is a similar force which governs all those motions, or the explanation may involve nothing more than a single identity, as when we explain the appearance of shooting stars by showing that they are identical with portions of a comet. Wherever we detect resemblance, there is a more or less satisfactory explanation. The mind is always somewhat disquieted when it meets a novel phenomena, one which is *sui generis;* it seeks at once for any parallels which may be found in the memory of past sensations. The so-called sulphurous smell which attends a stroke of lightning long excited the attention and fears of men, and it was not explained, until the exact similarity of the smell to that of ozone, or allotropic oxygen, was pointed out. The marks upon a flagstone are explained when they are shown to correspond with the feet of an extinct animal, whose bones are elsewhere found. Explanation, in fact, generally commences by the discovery of some very simple resemblance; the theory of the rainbow began as soon as Antonio de Dominis pointed out the resemblance between its colours and those presented by a ray of sunlight passing through a glass globe full of water.

The nature and limits of explanation can only be fully considered, after we have entered upon the subject of generalization and analogy. It must suffice to remark, in this place, that the most important process of explanation consists in showing that an observed fact is only one case of a general law or tendency. Iron is always found combined with sulphur, when it is in contact with or included in coal, whereas in other parts of the coal strata it always occurs as a carbonate. We explain this empirical fact as being due to the ordinary reducing powers of carbon and hydrogen, which prevent the iron from combining with oxygen, and leave it open to the affinity of the sulphur.

The uniform direction and strength of the trade-winds were long familiar to mariners, before they were explained by Halley on hydrostatical principles. The winds were found to arise from the action of gravity, which causes any heavy body to displace a lighter one, while the direction from east to west was also explained as a necessary result of the earth's rotation. Whatever body moves in the northern hemisphere from north to south, whether it be a bird, or a railway train, or a body of air, must tend towards the right hand, or west. Dove's law of the winds is to the effect that the winds tend to veer in the northern hemisphere in the direction N.E.S.W., and in the southern hemisphere in the direction N.W.S.E. This tendency was shown by him to be the necessary effect of the same conditions which apply to the trade-winds. Whenever, then, any fact is connected by resemblance, law, theory, hypothesis, or any other process of reasoning, with other facts, it is explained.

Although the great mass of recorded facts must be empirical, and awaiting explanation, such knowledge is of minor value, because it does not admit of extensive and safe inference. Each recorded result informs us exactly what will be experienced again in the same circumstances, but has no bearing upon what will happen in other circumstances.

Overlooked Results of Theory.

We must by no means suppose that, even when a scientific truth is firmly in our possession, all its consequences will be foreseen. Deduction is, as I have frequently remarked, certain and infallible, in the sense that each step in deductive reasoning will lead us to some result, as certain as the law itself. But it does not follow *that* every mode of deducing a fact from a law, or a combination of laws, will occur to a reasoner. Whatever road a traveller takes, he is sure to arrive somewhere, but unless he proceed in a very systematic manner, it is very unlikely that he will reach every place to which a network of roads will conduct him. In like manner there are many phenomena which were virtually within the reach of philosophers by inference from their previous knowledge, but were never discovered until accident or systematic empirical observation disclosed their existence.

That light is propagated with a certain uniform but very high velocity, was proved by Roemer, by observation of the eclipses of Jupiter's satellites. Corrections could henceforward be made in all astronomical observations requiring it, for the difference of absolute time at which an event happens, and that at which it becomes evident to us. But no person happened to remark that the motion of light compounded with that of the earth in its orbit would occasion a small apparent displacement of the greater part of the heavenly bodies. Fifty years elapsed before Bradley empirically discovered this effect, called by him aberration, when examining his accurate observations of the fixed stars[i].

When once the relation between an electric current and a magnet had been detected by Oersted and Faraday, it ought, theoretically speaking, to have been possible for them or any other person to foresee the diverse results which must ensue in different circumstances. If, for instance, a plate of copper were placed beneath an oscillating magnetic needle it should have been seen that the needle would induce currents in the copper, but as this could not take place without a certain reaction against the needle, it ought to have been seen that the needle would come to rest more rapidly than in the absence of the copper. Yet this peculiar effect was accidentally discovered by Gambey in 1824. Arago acutely inferred from Gambey's experiment that if the copper were set in rotation while the needle was stationary the motion would gradually be communicated to the needle. The phenomenon nevertheless puzzled the whole scientific world, and it required the deductive genius of Faraday to show that it was a necessary result of the principles of electro-magnetism[k]. By an act of deductive reasoning Faraday anticipated that a piece of copper rotating between the poles of a powerful magnet must experience a kind of resistance which will soon bring it to rest, and this effect he proved to exist in a decisive experiment[l].

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[i] Laplace, 'Précis de l'histoire de l'Astronomie,' p. 104.

Many other curious facts might be mentioned which when once noticed were explained as the effects of well-known natural laws. It was accidentally discovered that the navigation of canals of small depth could be greatly facilitated by increasing the speed of the boats, the resistance being actually reduced by this increase of speed, which enables the boat to ride as it were upon its own forced wave. Now mathematical theory might have predicted this result had the right application of the formulæ occurred to any one[m]. Giffard's injector for supplying steam boilers with water by the force of their own steam, was, I believe, accidentally discovered, but no new principles of mechanics are involved in it, so that it might have been theoretically invented. The same may be said of the curious experiment in which a stream of air or steam issuing from a pipe is made to hold a free disc upon the end of the pipe and thus apparently obstruct its own free outlet. The possession then of a true theory does not by any means imply the foreseeing of all the results. The effects of even a few simple laws may be infinitely diverse, and some of the most curious and useful effects may remain undetected until accidental observation brings them to our notice.

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[k] 'Experimental Researches in Electricity,' 1st Series, pp. 24–44. Paragraphs 81–139.

[l] Jamin, 'Cours de Physique,' tom. iii. p. 297.

[m] Airy, 'On Tides and Waves,' Encyclopædia Metropolitana, p. 348 *.

Predicted Discoveries.

The most interesting of the four classes of facts or phenomena as specified in p. 157, is probably the third—containing those the occurrence of which has been first predicted by theory, and then verified by observation. There is no more convincing proof of the soundness of scientific knowledge than that it thus confers the gift of foresight. Auguste Comte said that 'Prevision is the test of true theory;' I should say that it is only one test of true theory, but that which is most likely to strike the public attention. Coincidence with fact is the test of true theory, but when the result of theory is announced beforehand, there can be no possible doubt as to the unprejudiced and confident spirit in which the theorist interprets the results of his own theory.

The earliest instance of scientific prophecy is naturally furnished by the science of Astronomy, which was the earliest in development. Herodotus narrates[n] that, in the midst of a battle between the Medes and Lydians, the day was suddenly turned into night, and the event had been foretold by Thales, the Father of Philosophy. A cessation of the combat and a peace confirmed by marriages was the immediate consequence of this happy scientific effort. Much controversy has taken place concerning the exact date of this occurrence, Baily assigning the year 610 B.C., but Sir G. B. Airy has lately decided that the exact day was the 28th of May, 584 B.C.

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[n] Lib. i. cap. 74.

There can be no doubt that this and other predictions of eclipses attributed to ancient philosophers were due to an obscure knowledge of the Metonic Cycle, a period of 6585 days, or 223 lunar months, or about 19 years in which a nearly perfect recurrence of the phases and eclipses of the moon takes place; but if so, Thales must have had access to a long series of astronomical records either those of the Egyptians or the Chaldeans. There is a well known story as to the happy use which Columbus made of the power of predicting eclipses in overawing the islanders of Jamaica who refused him necessary supplies of food for his fleet. He threatened to deprive them of the moon's light. 'His threat was treated at first with indifference, but when the eclipse actually commenced, the barbarians vied with each other in the production of the necessary supplies for the Spanish fleet.'

Exactly the same kind of interest and awe which the ancients experienced at the prediction of eclipses, has been felt in modern times concerning the return of comets. Seneca indeed asserted in most distinct and remarkable terms that comets would be found to revolve in periodic orbits and return to sight. The ancient Chaldeans and the Pythagoreans are also said to have entertained a like opinion. But it was not until the age of Newton and Halley that it became possible to calculate the path of a comet in future years. A great comet appeared in 1682, a few years before the first publication of the 'Principia,' and Halley showed that its orbit corresponded with those of remarkable comets rudely recorded to have appeared in the years 1531 and 1607. The intervals of time indeed were not quite equal, but Halley conceived the bold idea that this difference might be due to the disturbing power of Jupiter, near which great planet the comet had passed in the interval 1607–1682. He predicted that the comet *would* return about the end of 1758 or the beginning of

1759, and though Halley did not live to enjoy the sight, it was actually detected on the night of Christmas-day, 1758. A second return of the comet was witnessed in 1835 nearly at the time anticipated.

In recent times the discovery of Neptune has been the most remarkable instance of prevision in astronomical science. A full account of this discovery may be found in several works, as for instance Herschel's 'Outlines of Astronomy' and 'Grant's History of Physical Astronomy,' Chapters XII and XIII.

Predictions in the Science of Light.

Next after astronomy the science of physical optics has furnished the most beautiful and early instances of the prophetic power of correct theory. These cases are the more striking because they proceed from the profound application of mathematical analysis, and show an insight into the mysterious workings of matter which is surprising to all, but especially to the great majority of men who are unable to comprehend the methods of research employed. By its power of prevision the truth of the undulatory theory of light has been conspicuously proved, and it is especially to be remarked that even Newton received no assistance from his Corpuscular theory in the detection of new experiments. To his followers who embraced that theory we owe little or nothing in the science of light, and even the lofty genius of Laplace did not derive from it a single discovery. As Fresnel himself remarks[o] :—

'The assistance to be derived from a good theory is not to be confined to the calculation of the forces when the laws of the phenomena are known. There are certain laws so complicated and so singular, that observation alone, aided by analogy, could never lead to their discovery. To divine these enigmas we must be guided by theoretical ideas founded on a *true* hypothesis. The theory of luminous vibrations presents this character, and these precious advantages; for to it we owe the discovery of optical laws the most complicated and most difficult to divine.'

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[o] Taylor's 'Scientific Memoirs,' vol. v. p. 241.

Physicists who embraced the barren emission theory had nothing but their own native capacity and quickness of observation to rely upon. Fresnel having once seized the conditions of the true undulatory theory, as previously stated by Young, was enabled by the mere manipulation of his mathematical symbols to foresee many of the complicated phenomena of light. Who could possibly suppose, or even believe on the ground of mere common sense, that by stopping a large portion of the rays passing through a circular aperture, the illumination of a point upon a screen behind the aperture might be many times multiplied. Yet this paradoxical effect was predicted by Fresnel, and verified both by himself, and in a careful repetition of the experiment in later years, by Billet. Comparatively few persons even now are aware that in the very middle point of the shadow of an opaque circular disc is a point of light sensibly as bright as if no disc had been interposed. This startling fact was deduced from Fresnel's theory by Poisson, and was then verified experimentally by Arago. Airy, again, was led by pure theory to predict that Newton's rings would present a modified appearance if produced between a lens of glass and a plate of metal. This effect happened to have been observed fifteen years before by Arago, unknown to Airy; but another prediction of Airy, that there would be a further modification of the rings when made between two substances of very different refractive indices, was verified by subsequent *trial* with a diamond. A reversal of the rings takes place when the space intervening between the plates is filled with a substance of intermediate refractive power, another phenomenon predicted by theory and verified by experiment as Sir John Herschel has described. There is hardly a limit to the number of other complicated effects of the interference of rays of light under different circumstances which might be deduced from the mathematical expressions, if it were worth while, or which, being previously observed can be explained, as in an interesting case observed by Sir John Herschel and explained by Airy[p].

By a somewhat different effort of scientific foresight, Fresnel discovered that any solid transparent medium might be endowed with the power of double refraction by mere compression. For as he attributed the peculiar refracting power of crystals to the unequal elasticity in different directions, he inferred that unequal elasticity, if artificially produced, would give similar phenomena. With a powerful screw and a piece of glass, he then produced not only the colours due to double refraction, but the actual duplication of images. Thus, by a great scientific generalisation, are the apparently unique properties of Iceland spar shown to belong to all transparent substances under certain conditions[q].

All other predictions in optical science are, however, thrown into the shade by the theoretical discovery of conical refraction by the late Sir W. R. Hamilton, of Dublin. In investigating the passage of light through certain crystals, Hamilton found that Fresnel had slightly misinterpreted his own formulæ, and that, when rightly understood, they indicated a phenomenon of a kind never witnessed. A small ray of light sent into a crystal of arragonite in a particular direction, becomes spread out into an infinite number of rays, which form a hollow cone within the crystal, and a hollow cylinder when emerging from the opposite side. In another case, a somewhat different, but equally strange, effect is produced. These phenomena are peculiarly interesting, because cones and cylinders of light are not produced in any other cases. They are, in fact, wholly opposed to all analogy, and constitute singular, or exceptional cases, of a kind which we shall afterwards have to consider more fully. Their very strangeness rendered them peculiarly fitted to test the truth of the theory by which they were discovered; and when Professor Lloyd, at Hamilton's request, succeeded, after considerable difficulty, in witnessing the new appearances, no further doubt could remain of the validity of the great theory of waves, which we owe to Huyghens, Young, and Fresnel[r].

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[p] Airy's, 'Mathematical Tracts,' 3rd edit. p. 312.

[q] Young's 'Works,' vol. i. p. 412.

Predictions from the Theory of Undulations.

It is curious to reflect that the undulations of light, although so inconceivably rapid and small, admit of more accurate observation and measurement than the waves of any other medium. But so far as we can carry out exact experiments on other kinds of waves, we find the phenomena of interference repeated, and analogy gives considerable powers of prediction. Sir John Herschel was perhaps the first to suggest that two sounds might be made to destroy each other by interference[s]. For if one-half of a wave travelling through a tube could be separated, and conducted by a somewhat longer passage, so as, on rejoining the other half, to be one-quarter of a vibration behindhand, the two portions would exactly neutralise each other. This experiment has recently been performed with success by Quincke and König[t]. The interference arising between the waves from the two prongs of a tuning-fork was also predicted by theory, and proved to exist by Weber; indeed it may be observed by merely turning round a vibrating fork close to the ear[u].

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[r] Lloyd's 'Wave Theory,' Part ii. pp. 52–58. Babbage, 'Ninth Bridgwater Treatise,' p. 104, quoting Lloyd, 'Trans. of the Royal Irish Academy,' vol. xvii. Clifton, 'Quarterly Journal of Pure and Applied Mathematics,' January, 1860.

[s] 'Encyclopædia Metropolitana,' art. *Sound*, p. 753.

It is a plain result of the theory of sound that, if we move rapidly towards a sounding body, or if it move rapidly towards us, the pitch of the sound will be a little more acute; and, *vice versâ*, when the relative motion is in the opposite direction, the pitch will be more grave. It arises from the less or greater intervals of time between the successive strokes of waves upon the auditory nerve, according as the ear moves towards or from the source of sound relatively speaking. This effect was predicted by theory, and afterwards verified by the experiments of M. Buys Ballot, on Dutch railways, and of Mr. Scott Russell, in England[x]. Whenever, indeed, one railway train passes another, on the locomotive of which the whistle is being sounded, the drop in the acuteness of the sound may be noticed at the moment of passing. This change gives the sound a peculiar howling character, which many persons must have noticed. I have calculated that, with two trains travelling thirty miles an hour, the effect would amount to rather more than half a tone, and it would often amount to a tone. A corresponding effect is produced in the case of light undulations, when the eye and the luminous body rapidly approach or recede from each other. It is shown by a slight change in the refrangibility of the rays of light, and a consequent change in the place of the lines of the spectrum, which has been made to give most important and unexpected information concerning the relative approach or recession of many stars as regards the earth.

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[t] Tyndall's 'Sound,' p. 261.

[u] Ibid. p. 273.

[x] Ibid. p. 78.

Tides are vast waves, and were the earth's surface entirely covered by an ocean of uniform depth, they would admit of very exact theoretical investigation. The wholly irregular form of the several seas introduces unknown quantities and complexities with which theory cannot cope. Nevertheless, Whewell, observing that the tides of the German Ocean consist of interfering waves, which arrive partly round the north of Scotland and partly through the British Channel, was enabled to predict that at a point about midway between Lowestoft and Brill on the coast of Holland, in latitude 52° 27′ N, and longitude 3 h. 14 m. E, no tides would be found to exist. At that point the two waves would be of exactly the same amount, but in opposite phases, so as to neutralise each other. This assertion was verified by a surveying vessel of the British navy[y].

Prediction in other Sciences.

Generations, or even centuries, will probably elapse before mankind are in possession of a mathematical theory of the constitution of matter as complete and satisfactory as the theory of gravitation. Nevertheless, mathematical physicists have in recent years acquired a fair hold of some of the simple relations of the physical forces to matter, and the proof is found in some remarkable anticipations of curious phenomena which had never been observed. Professor James Thomson deduced from Carnot's theory of heat that the application of pressure would lower the melting-point of ice. He even ventured to assign the amount of this effect, and his statement was afterwards verified almost exactly by Sir W. Thomson[z]. 'In this very remarkable speculation, an entirely novel physical phenomenon was *predicted*, in anticipation of any direct experiments on the subject; and the actual observation of the phenomenon was pointed out as a highly interesting object for experimental research.' Just as liquids which expand in solidifying will have the temperature of solidification lowered by pressure, so liquids which contract in solidifying will exhibit the reverse effect. They will be assisted in solidifying, as it were, by pressure, so as to become solid at a higher temperature, as the pressure is greater. This latter result was verified by Bunsen and Hopkins, in the case of paraffin, spermaceti, wax, and stearin. The effect upon water has more recently been carried to such an extent by Mousson, that under the vast pressure of 1300 atmospheres, water did not freeze until cooled down to -18° Cent. Another remarkable prediction of Professor Thomson was to the effect that, if a metallic spring be weakened by a rise of temperature, work done against the spring, by bending it, must cause a cooling effect. Although the amount of effect to be expected in a certain apparatus was only about four-thousandths of a degree Centigrade, Dr. Joule[a] succeeded in detecting and measuring the effect to the extent of three-thousandths of a degree, such is the delicacy of modern methods of measurement. I cannot refrain from quoting Dr. Joule's reflections upon this fact[b]. 'Thus even in the above delicate case,' he says, 'is the formula of Professor Thompson completely verified. The mathematical investigation of the thermo-elastic qualities of metals has enabled my illustrious friend to predict with certainty a whole class of highly interesting phenomena. To him especially do we owe the important advance which has been recently made to a new era in the history of science, when the famous philosophical system of Bacon will be to a great extent superseded, and when, instead of arriving at discovery by induction from experiment, we shall obtain our largest accessions of new facts by reasoning deductively from fundamental principles.'

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[y] Whewell's 'History of the Inductive Sciences,' vol. ii. p. 471. *Herschel's* 'Physical Geography,' § 77.

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[z] Maxwell's 'Theory of Heat,' p. 174. 'Philosophical Magazine,' August, 1850. Third Series, vol. xxxvii. p. 123.

[a] 'Philosophical Transactions,' 1858, vol. cxlviii. p. 127.

[b] Ibid. p. 130.

The theory of electricity is a necessary part of the general theory of matter, and is rapidly acquiring the power of prevision. As soon as Wheatstone had proved experimentally that the conduction of electricity occupied time, Faraday remarked in 1838, with wonderful sagacity, that if the conducting wires were connected with the coatings of a large Leyden jar, the rapidity of conduction would be lessened. This prediction remained unverified for sixteen years, until the submarine cable was laid beneath the Channel. A considerable retardation of the electric spark was then detected by Siemens and Latimer Clark, and Faraday at once pointed out that the wire surrounded by water resembles a Leyden jar on a large scale, so that each message sent through the cable verified his remark of 1838[c].

The joint relations of heat and electricity to the metals constitute almost a new science of thermo-electricity. Sir W. Thompson was enabled by theory to anticipate the following curious effect, namely, that an electric current passing in an iron bar from a hot to a cold part produces a cooling effect, but in a copper bar the effect is exactly opposite in character, that is the bar becomes heated[d]. The action of crystals with regard to heat and electricity was partly foreseen on the grounds of theory by Poisson.

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[c] Tyndall's 'Faraday,' pp. 73, 74; 'Life of Faraday,' vol. ii. pp. 82, 83.

[d] *Tait's* 'Thermodynamics,' p. 77.

Chemistry, although to a great extent an empirical science, has not been without prophetical triumphs. The existence of the metals potassium and sodium was foreseen by Lavoisier, and their elimination by Davy was one of the chief *experimenta crucis* which established Lavoisier's system. The existence of many other metals which eye had never seen was almost a necessary inference, and theory has not been found at fault. No sooner, too, had a theory of organic compounds been conceived by Professor A. W. Williamson than he foretold the formation of a complex substance consisting of water in which both atoms of hydrogen are replaced by atoms of acetyle. This substance, known as the acetic anhydride, was afterwards produced by Gerhardt. In the subsequent progress of organic chemistry occurrences of this kind have been multiplied almost indefinitely. The theoretical chemist by the classification of his specimens and the manipulation of his formulæ can plan out as it were the creation of whole series of unknown oils, acids, alcohols, and such like products, just as a designer might draw out a multitude of patterns. The formation of many such substances is a matter of course, but there is an interesting prediction given by Hofmann, concerning the possible existence of new compounds of sulphur and selenium, and even oxides of ammonium, which it remains for the future to verify[e].

Prediction by Inversion of Cause and Effect.

There is one process of experiment which has so often led to important discoveries as to deserve separate description and illustration—I mean the inversion of Cause and Effect. Thus if A and B in one experiment produce C as a consequent, then antecedents of the nature of B and C may usually be made to produce a consequent of the nature of A inverted in direction. When we apply heat to a gas it tends to expand; hence if we allow the gas to expand by its own elastic force, cold is the result; that is B (air) and C (expansion) produce the negative of A (heat). Or again, B (air) and compression, the negative of C, produce A (heat). Similar results may be expected in a multitude of cases. It is a most familiar law that heat expands iron and nearly all solid bodies. What may be expected, then, if instead of increasing the length of an iron bar by heat we use mechanical force and stretch the bar? Having the bar and the former consequent, expansion, we should expect the negative of the former antecedent, namely cold. The truth of this inference was proved by Dr. Joule, who investigated the amount of the effect with his usual skill[f].

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[e] Hofmann's 'Introduction to Chemistry,' pp. 224, 225.

This inversion of cause and effect in the case of heat may be itself again inverted in a highly curious manner. It happens that there are a few substances which are unexplained exceptions to the general law of expansion by heat. India-rubber especially is remarkable for *contracting* when heated. Since, then, iron and india-rubber are oppositely related to heat, we may expect that as distension of the iron produced cold, distension of the india-rubber will produce heat. This is actually found to be the case, and any one may detect the effect by suddenly stretching an india-rubber band while the middle part is in the mouth. Whenever stretched it will be found to grow slightly warm, and when relaxed cold.

The reader will readily see that many of the scientific predictions mentioned in preceding sections were due to the principle of inversion; for instance, Professor Thompson's speculations on the relation of pressure and the melting-point. But many other illustrations could be adduced. The usual agent by which we melt or liquefy a substance is heat; but if we can melt a substance without heat, then we may expect the negative of heat as an effect. This is the foundation of all freezing mixtures. The affinity of salt for water causes it to melt snow or ice, and may thus reduce the temperature to Fahrenheit's zero. Calcium chloride has so much higher an attraction for water that a temperature of −50° Fahr. may thus be attained. Even the solution of a certain alloy of lead, tin, and bismuth in mercury, may be made to reduce the temperature from 63° to 14° Fahr. All the other modes of producing cold are inversions of more familiar uses of heat. Carré's freezing machine is an inverted distilling apparatus, the distillation being occasioned by chemical affinity instead of heat. Another kind of freezing machine is the exact inverse of the steam engine.

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[f] 'Philosophical Transactions,' (1855) vol. cxlv. pp. 100, &c.

A very paradoxical effect is due to another inversion. It is hard to believe at the first moment that a current of steam at 212° could raise a body of liquid to a higher temperature than the steam itself possesses. But Mr. Spence has pointed out that if the boiling-point of a saline solution be above 212°, it will continue, on account of its affinity for water, to condense steam when above 212° in temperature. It will condense the steam until heated to the point at which the tension of its vapour is equal to that of the atmosphere, that is, its own boiling-point[g].

Since heat, again, melts ice, we might expect to produce heat by the inverse change from water into ice. Now this is accomplished in the phenomenon of suspended freezing. Water may be cooled in a clean glass vessel many degrees below the freezing-point, and yet retained in the liquid condition. But if disturbed, and especially if brought into contact with a small particle of ice, it immediately solidifies and rises in temperature to 32° Fahr. A like effect is still more beautifully displayed in the well known lecture-room experiment, of the suspended crystallization of a solution of sodium sulphate, in which a sudden rise of temperature of 30° or even 40° Fahr. is often manifested.

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[g] 'Proceedings of the Manchester Philosophical Society,' Feb. 1870.

The science of electricity is full of the most varied and interesting cases of inversion. As Professor Tyndall has remarked, Faraday had a profound belief in the reciprocal relations of the physical forces. The great starting-point of his researches, the discovery of electro-magnetism, was clearly an inversion.

Oersted and Ampère had proved that with an electric current and a magnet in a particular position as antecedents, motion is the consequent. If then a magnet, a wire and *motion* be the antecedents, an *opposite* electric current will be the consequent. It would be an endless task to trace out the results of this fertile relationship when once fully understood. No small part of Faraday's researches was occupied in ascertaining the direct and inverse relations of magnetic and diamagnetic, amorphous and crystalline substances in various circumstances. In all other relations of electricity the principle of inversion holds. The voltameter or the electro-plating cell is the inverse of the galvanic battery. As heat applied to a junction of antimony and bismuth bars produces electricity, it necessarily follows that an electric current passed through such a junction will produce cold. Thus it is apparent that inversion of cause and effect is a most fertile ground of prediction and discovery.

The reader should carefully notice, however, that the inversion of natural phenomena is exactly true only of the character of the effect, not the amount. There is always a waste of energy in every work, because a certain part of it is dissipated in the form of conducted or radiated heat, and escapes beyond our use. Theoretically speaking, we might imagine a train of magnetic engines and electro-magnetic machines, which should alternately convert the same energy into motion and electricity. Similarly, by a proper arrangement of bars of antimony and bismuth, the same current of electricity might be converted into heat and reconverted into electricity an indefinite number of times. But, practically speaking, there would be an enormous loss of energy at each conversion, so that the ultimate effect would dwindle down to an inconsiderable fraction of the original amount of energy.

Facts known only by Theory.

Of the four classes of facts enumerated in p. 157 the last remains unconsidered. It includes the unverified predictions of science. Scientific prophecy arrests the attention of the world when it refers to such striking events as an eclipse, the appearance of a great comet, or any other phenomenon which every one can verify with his own eyes. But it is surely a greater matter for wonder that in many cases a physicist may describe and measure phenomena which eye cannot see, nor sense of any kind appreciate. In most cases this arises from the effect being too small in amount to affect our organs of sense, or come within the powers of our instruments as at present constructed. There is another class of yet more remarkable cases, where a phenomenon cannot possibly be observed, and yet we can say what it would be if it were observed.

In astronomy, systematic aberration is an effect of the sun's proper motion almost certainly known to exist, but which we have no hope of detecting by observation in the present age of the world. As the earth's motion round the sun combined with the motion of light causes the stars to deviate apparently from their true positions to the extent of about 18″ at the most, so the motion of the whole planetary system through space must occasion a similar displacement of at most 5″. The ordinary aberration can be readily detected with modern astronomical instruments, because it goes through a yearly change in direction or amount, but the systematic aberration is constant and permanent so long as the planetary system moves uniformly in a sensibly straight line. Only then in the course of ages, when the curvature of the sun's path becomes apparent, can we hope to verify the existence of this kind of aberration. A curious effect also must be produced by the sun's proper motion upon the apparent periods of revolution of the binary stars.

To my mind, some of the most interesting truths in the whole range of science are those which have not been, and in many cases probably can never be, verified by trial. Thus the chemist assigns, with a very high degree of probability the vapour densities of such elements as carbon and silicon, which have never been observed separately in a state of vapour. The chemist also is familiar with the vapour densities of elements at temperatures at which the elements in question never have been, and probably never can be, submitted to experiment in the form of vapour.

Joule and others have calculated the actual velocity of the molecules of a gas, and even the number of collisions which must take place per second during their constant circulation. Sir W. Thomson has not yet given us the exact absolute magnitudes of the particles of matter, but he has ascertained by several distinct methods the limits within which their magnitudes must lie. Many of such scientific results must for ever be beyond the power of verification by the senses. I have elsewhere had occasion to remark that waves of light, the intimate processes of electrical changes, the properties of the ether which is *the base* of all phenomena, are necessarily determined in a hypothetical, but not therefore a less certain manner.

Though only two of the metals, gold and silver, have ever been observed to be transparent, we know on the grounds of theory that they are all more or less so; we can even estimate by theory their refractive indices, and prove that they are exceedingly high. The phenomena of elliptic polarization, and perhaps also the theory of internal radiation[h], depend upon the refractive index, and thus, even when we cannot observe any refracted rays, we can indirectly learn how they would be refracted.

In many cases large quantities of electricity must be produced, which we cannot observe because it is instantly discharged. In the common electric machine the cylinder and rubber are made of non-conductors, so that we can separate and accumulate the electricity. But even a little damp, by serving as a conductor, prevents this separation from enduring any sensible time. Hence there is little or no doubt that when we rub two good conductors against each other, for instance two pieces of metals, much electricity is produced, but instantaneously converted into some other form of energy. Dr. Joule, indeed, believes that all the heat of friction is but transmuted electricity.

As regards phenomena of insensible amount, Nature is absolutely full of them. We must, in fact, regard those considerable changes which we can observe as the comparatively speaking infinitely rare aggregates of minuter changes. On a little reflection we must allow that no object known to us remains for two instants of exactly the same temperature. If so, the dimensions of objects must be in a perpetual state of variation. The minor planetary and lunar perturbations are indefinitely, or rather infinitely numerous, but usually too small to be detected by

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[h] Balfour Stewart, 'Elementary Treatise on Heat,' 1st edit. p. 198.

(5) We may indirectly predict or determine the quantity of an effect without being able to verify it by experiment.

These various classes of quantitative facts might be illustrated by an almost infinite number of interesting points in the history of physical science. Philosophical prophecies especially serve to show the mastery which is sometimes attained over the secrets of nature, and to convince the least intelligent of the value of theory.

Empirical Measurements.

Under the first head of purely empirical measurements, which have not been brought under any theoretical system, may be placed the great bulk of quantitative facts recorded by scientific observers. The tables of numerical results which abound in books on chemistry and physics, the huge quartos containing the observations of public observatories, the multitudinous tables of meteorological observations, which are continually being compiled and printed, the more abstruse results concerning terrestrial magnetism—such results of measurement, for the most part, remain empirical, either because theory is defective, or the labour of calculation and comparison is too formidable. In the Greenwich Observatory, indeed, the salutary practice has been maintained by the present Astronomer Royal, of always reducing the observations, and comparing them with the recognised theories of motion of the several bodies. The divergences from theory thus afford a constant supply of material for the discovery of errors or of new phenomena; in short, the observations have been turned to the use for which they were intended. But it is to be feared that other establishments are too often engaged in merely recording numbers of which no real use is made, because the labour of reduc-

CHAPTER XXV.

ACCORDANCE OF QUANTITATIVE THEORIES AND EXPERIMENTS.

In the preceding chapter we found that facts may be classed under four heads as regards their connexion with theory, and our powers of explanation or prediction. The facts hitherto considered were generally of a qualitative rather than a quantitative nature; but when we look exclusively to the quantity of a phenomenon, and the various modes in which we may estimate or establish its amount, almost the same system of classification will hold good. There will, however, be five possible cases:—

(1) We may directly and empirically measure a phenomenon, without being able to explain why it should have any particular quantity, or to connect it by theory with other quantities.

(2) In a considerable number of cases we can theoretically predict the existence of a phenomenon, but may be unable to assign its amount, except by direct measurement, or to explain the amount theoretically when thus ascertained.

(3) We may measure a quantity, and afterwards explain it as related to other quantities, or as governed by known quantitative laws.

(4) We may predict the quantity of an effect on theoretical grounds, and afterwards confirm the prediction by direct measurement.

(5) We may indirectly predict or determine the quantity of an effect without being able to verify it by experiment.

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Purely empirical measurements may often indeed have a direct practical value, as when tables of the specific gravity, or strength of materials, assist the engineer; the specific gravities of mixtures of water with acids, alcohols, salts, &c., are useful in chemical manufactories, customhouse guaging, &c.; observations of rain-fall are requisite for questions of water supply; the refractive index of various kinds of glass must be known in making achromatic lenses; but in all such cases the use made of the measurements is not scientific, but practical. It may probably be asserted with truth, that no number which remains entirely isolated, and uncompared by theory with other numbers, is of any really scientific value. Having tried the tensile strength of a piece of iron in a particular condition, we know what will be the strength of the same kind of iron in a similar condition, provided we can ever meet with that exact kind of iron again; but we cannot argue from piece to piece, or lay down any laws exactly connecting the strength of iron with the quantity of its impurities.

It is to be feared that almost the whole bulk of statistical numbers, whether commercial, vital, or moral, is at present, and probably will long continue, of little scientific value.

Quantities indicated by Theory, but Empirically Measured.

In many cases we are able to foresee the existence of a quantitative effect, on the ground of general principles, but are unable, either from the want of numerical data, or from the entire absence of any mathematical theory, to assign the amount of such effect. We then have recourse to direct experiment to determine its amount. Whether we argued from the oceanic tides by analogy, or more generally from the theory of gravitation, there could be no doubt that atmospheric tides of some amount, depending on the apparent heights of the sun and the moon, must occur in the atmosphere. Theory, however, even in the hands of Laplace, was not able to overcome the complicated mechanical conditions of the atmosphere, and predict the amount of such tides; and, on the other hand, these amounts were so small, and were so masked by far larger undulations arising from the heating power of the sun, and from other meteorological disturbances, that they would probably have never been discovered by purely empirical observations. Theory having, however, indicated their existence, it was easy to make series of barometrical observations in places selected so as to be as free as possible from casual fluctuations, and then by the suitable application of the method of means to detect the small effects in question. The principal lunar atmospheric tide was thus proved to amount to between ·003 and ·004 inch[a].

Theory, in fact, yields the greatest possible assistance in applying the method of means. For if we have a great number of empirical measurements, each representing the joint effect of a number of causes, our object will be to take the mean of all those in which the effect to be measured is present, and compare it with the mean of the remainder in which the effect is absent, or acts, it may be, in the opposite direction. The difference will then represent the amount of the effect, or double the amount respectively. Thus, in the case of the atmospheric tides, we take the mean of all the observations when the moon was on the meridian, and compare it with the mean of all observations when she was on the horizon. In this case we trust to chance that all other effects will lie about as often in one direction as the other in the drawing of each mean, and will neutralise themselves. It will be a great advantage, however, to be able to decide by theory when each principal disturbing effect is present or absent; for the means may then be so drawn as surely to separate each such effect, leaving only very minor and casual divergences to the law of error. Thus, if there be three principal effects, and we draw means giving respectively the sum of all three, the sum of the first two, and the sum of the last two, then we gain three simple equations, by the solution of which each quantity is determined.

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[a] Grant's 'History of Physical Astronomy,' p. 162.

Explained Results of Measurement.

The second class of measured phenomena contains those which, after being determined in a direct and purely empirical application of measuring instruments, are afterwards shown to agree with some hypothetical explanation. Such results are turned to their proper use, and several different advantages may arise from the comparison. The correspondence with theory will seldom or never be absolutely precise; and, even if it be so, the coincidence must be regarded as accidental. If the divergences between theory and experiment be comparatively small, and variable in amount and direction, they may often be safely attributed to various inconsiderable sources of error in the experimental processes. The strict method of procedure is to calculate, if possible, the probable error of the mean of the observed results (vol. i. p. 451), and then observe whether the theoretical result falls within the limits of probable error. If it does, and if, as we may say, the experimental results agree as well with theory as they agree with each other, then the probability of the theory is much increased, and we may employ the theory with more confidence in the anticipation of further results. The probable error, it should be remembered, gives a measure only of the effects of incidental and variable sources of error, but in no way or degree indicates the amount of fixed causes of error. Thus, if the mean results of any two modes of determining a quantity are so far apart that the limits of probable error do not overlap, we may infer the probable existence of some overlooked source of permanent error in one or both modes. We will further consider in a subsequent section the accordance or discordance of measurements.

Quantities determined by Theory and verified by Measurement.

One of the most satisfactory tests of a theory consists in its application not only to predict the nature of a phenomenon, and the circumstances in which it may be observed, but also to assign the precise quantity of the phenomenon. If we can subsequently apply accurate instruments and measure the amount of the phenomenon witnessed, we have an excellent opportunity of verifying or negativing the theory. It was in this manner that Newton first attempted to verify his theory of gravitation. He knew approximately the velocity produced in falling bodies at the earth's surface, and if the law of the inverse square of the distance held true, and the reputed distance of the moon was correct, he could infer that the moon ought to fall towards the earth at the rate of fifteen feet in one minute. Now, the actual divergence of the moon from the tangent of its orbit appeared to amount only to thirteen feet in one minute, and there was a discrepancy of two feet in fifteen, which caused Newton to lay 'aside at that time any further thoughts of this matter.' Many years afterwards, probably fifteen or sixteen years, Newton obtained more precise data from which he could calculate the size of the moon's orbit, and he then found the discrepancy to be inconsiderable.

His theory of gravitation was then verified so far as the moon was concerned; but this was to him only the beginning of a long course of deductive calculations, each ending in a verification. If the earth and moon attract each other, and also the sun and the earth, similarly there is no reason why the sun and moon should not attract each other. Newton followed out the consequences of this inference, and showed that the moon would not move as if attracted by the earth only, but sometimes faster and sometimes slower. Comparisons with Flamsteed's observations of the moon showed that such was the case. Newton argued again, that as the waters of the ocean are not rigidly attached to the earth, they might attract the moon, and be attracted in return, independently of the rest of the earth. Certain daily motions would then be caused thereby exactly resembling the tides, and there were the tides to verify the fact. It was the almost superhuman power with which he traced out geometrically the consequences of his theory, and submitted them to repeated comparison with experience, which constitutes his pre-eminence over all philosophers[b].

The whole progress of physical astronomy has consisted in a succession of predictions grounded on the theory of gravitation as to the inequalities of the planetary movements caused by mutual perturbations. These inequalities are so numerous, so small, and so complicated in character, that it would be an almost hopeless task to attempt to discover them empirically or tentatively by the comparison and classification of observations. But theory pretty easily indicates the period and general nature of the inequality to be detected, and by elaborate calculations even the amount of the effect may be assigned. Thus the inequality arising from the attraction of Venus and the earth was estimated by Sir George Airy to amount to no more than a few seconds at its maximum, while the period is no less than 240 years. Nevertheless, the indirect effects of this inequality upon the moon's motion are considerable, and are entirely verified in the lunar theory. Although prediction by theory is the general rule in physical astronomy, yet the empirical investigation of divergences from theory sometimes discloses effects which had been overlooked, or points out residual effects of unknown origin.

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[b] 'Elementary Lessons in Logic,' p. 262.

Quantities determined by Theory and not verified.

It will continually happen that we are able, from certain measured phenomena and a correct theory, to determine the amount of some other phenomenon which we may either be unable to measure at all, or to measure with an accuracy corresponding to that required to verify the prediction. Thus Laplace having worked out an almost complete theory of the motions of Jupiter's satellites on the hypothesis of gravitation, found that these motions were greatly affected by the spheroidal form of Jupiter. Hence from the motions of the satellites, which can be observed with great accuracy owing to the frequent eclipses and transits, he was able to argue inversely, and assign the ellipticity of the planet's section by theory. The ratio of the polar and equatorial axes thus determined was very nearly that of 13 to 14; and it agrees well with such direct micrometrical measurements of the planet as have been made; but Laplace believed that the theory gave a more accurate result than direct observation could yield, so that the theory could hardly be said to admit of direct verification.

The specific heat of air was believed on the grounds of direct experiment to amount to 0·2669, the specific heat of water being taken as unity; but the methods of experiment were open to considerable causes of error. The late Professor Rankine showed in 1850 that it was possible to calculate from the mechanical equivalent of heat, and from other thermodynamic data, what this number should be, and he found for it 0·2378. This determination was at the time accepted by him and others as the most satisfactory result, although not verified; subsequently in 1853 Regnault obtained by direct experiment the number 0·2377, proving that the prediction had been well grounded.

It will be readily seen that in purely quantitative questions verification will be a matter of degree and probability. A less accurate method of measurement cannot verify the results of a more accurate method, so that if we arrive at a determination of the same physical quantity in several distinct modes it will often become a delicate matter of investigation to decide which result is most reliable, and should be used for the indirect determination of other quantities. For instance, Joule's and Thomson's ingenious experiments upon the thermal phenomena of fluids in motion[c] involved, as one physical constant, the mechanical equivalent of heat; if requisite, then, they might have been used to predict or to correct that most important constant. But if other more direct methods of experiment give the mechanical equivalent of heat with superior accuracy, then the experiments on fluids will be turned to a better use in detecting and assigning various quantities relating to the theory of fluids. We will further consider questions of this kind in succeeding sections.

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[c] 'Philosophical Transactions' (1854), vol. cxliv. p. 364.

There are of course many quantities assigned on theoretical grounds which we are quite unable to verify with corresponding accuracy. The thickness of a film of gold leaf, the average depths of the oceans, the velocity of a star's approach to or regression from the earth as inferred from spectroscopic data, or other quantities indirectly determined (see vol. i. pp. 345–349), might be cases in point; but many others might be quoted where direct verification seems impossible. Newton and many subsequent physicists have accurately measured the lengths of light undulations, and by several distinct methods we learn the velocity with which light travels. Since an undulation of the middle green is about five ten-millionths of a metre in length, and travels at the rate of nearly 300,000,000 of metres per second, it necessarily follows that about 600,000,000,000,000 undulations must strike in one second the retina of an eye which perceives such light. But how are we to verify such an astounding calculation by directly counting pulses which recur six hundred billions of times in a second?

Discordance of Theory and Experiment.

When a distinct want of accordance is found to exist between the results of theory and direct measurement, several interesting questions may arise as to the mode in which we can account for this discordance. The ultimate explanation of the discrepancy may be accomplished in any one of at least four distinct ways, as follows :—

(1) The direct measurement may be erroneous owing to various sources of casual error.

(2) The theory may be correct so far as regards the general form of the supposed laws, but some of the constant numbers or other quantitative data employed in the theoretical calculations may be inaccurate.

(3) The theory may be false, in the sense that the forms of the mathematical equations assumed to express the laws of nature are incorrect.

(4) The theory and the involved quantities may be approximately accurate, but some regular unknown cause may have interfered, so that the divergence may be regarded as a *residual effect* representing possibly a new and interesting phenomenon.

No precise rules can be laid down as to the best mode of proceeding to explain the divergence, and the experimentalist will have to depend upon his own insight and knowledge; but the following general recommendations may perhaps be made.

In the first place, if the experimental measurements are not numerous, repeat them and take a more extensive mean result, the probable accuracy of which, as regards freedom from casual errors of experiment, will increase as the square root of the number of experiments. Supposing that no considerable modification of the result is thus effected, we may suspect the existence of some more deep-seated and constant source of error in our method of measurement. The next resource will be to change the size and form of the apparatus employed, and to introduce various modifications in the materials employed or in the course of procedure, in the hope, as before explained (vol. i. p. 462), that some cause of constant error may thus be removed. If the inconsistency with theory still remains unreduced we may attempt to invent some widely different mode of arriving at the same physical quantity, so that we may be almost sure that the same cause of error will not affect both the new and old results. In some cases it is possible to find five or six essentially different modes of arriving at the same determination.

Supposing that the discrepancy still exists we may well begin to suspect that our direct measurements are correct, but that the data employed in the theoretical calculations are inaccurate. We must now review the grounds on which these data depend, consisting as they must ultimately do of direct measurements. A comparison of the various recorded results will show the degree of probability attaching to the mean result employed; and if there is any ground for imagining the existence of error, we should repeat the observations, and vary the forms of experiment just as in the case of the previous direct measurements. The continued existence of the discrepancy must show that we have not really attained to a complete acquaintance with the theory of the causes in action, but two different cases still remain. We may have misunderstood the action of those causes which do exist, or we may have overlooked the existence of one or more other causes. In the first case our hypothesis appears to be wrongly chosen and inapplicable; but whether we are to reject it will depend upon whether we can form any other hypothesis which yields a more accurate accordance. The probability of an hypothesis, it will be remembered (vol. i. p. 279), is to be judged entirely by the probability that if the supposed causes exist the observed result follows; but as there is now very little probability of reconciling the original hypothesis with our direct measurements the field is open for new hypotheses, and any one which gives a closer accordance with measurement will so far have claims to attention. Of course we must never estimate the probability of an hypothesis merely by its accordance with a few results only. Its general analogy and accordance with other known laws of nature, and the fact that it does not conflict with any other probable theories, must be taken into account, as we shall see in the next book. The requisite condition of a good hypothesis, that it must admit of the deduction of facts verified in observation, must be interpreted in the widest possible manner, as including all ways in which there may be accordance or discordance.

All our attempts at reconciliation having failed, the only conclusion we can come to is that some unknown cause of a new character exists. If the measurements be accurate and the theory probable, then there remains a *residual phenomenon*, which, being devoid of theoretical explanation, must be set down as a new empirical fact worthy of deliberate investigation. As a matter of fact these outstanding residual discrepancies have often been found to involve new discoveries of the greatest importance.

Accordance of Measurements of Astronomical Distances.

One of the most instructive instances which we could meet, as regards the manner in which different measurements confirm or check each other, is furnished by the determination of the velocity of light, and the dimensions of the planetary system. Roemer first discovered that light requires time in travelling, by observing that the eclipses of Jupiter's satellites, although they of course occur at fixed moments of absolute time, are visible at different moments in different parts of the earth's orbit, according to the distance of the earth and Jupiter. The time occupied by light in traversing the mean semidiameter of the earth's orbit is found to be about eight minutes. The mean distance of the sun and earth was long assumed by astronomers as being about 95,274,000 miles, this result being deduced by Bessel from the observations of the transit of Venus, which occurred in 1769, and which were found to give the solar parallax, or what is the same thing, the apparent size of the earth as seen from the sun, as equal to 8″·578. Now, dividing the mean distance of the sun and earth by the number of seconds in $8^m. 13^s.3$ we find the velocity of light to be about 192,000 miles per second.

Nearly the same result was obtained in an apparently very different manner. The aberration of light is the apparent change in the direction of a ray of light owing to the composition of its motion with that of the earth's motion round the sun. If we know the amount of aberration and the mean velocity of the earth we can very simply estimate that of light which is thus found to be 191,102 miles (166,072 geographical miles) per second. Now this determination depends upon an entirely new physical quantity, that of aberration, which is ascertained by direct observation of the stars, so that the close accordance of the estimates of the velocity of light as thus arrived at by different methods might seem to leave little room for doubt, the difference being less than one per cent.

Nevertheless, experimentalists were not satisfied until they had succeeded in actually measuring the velocity of light by direct experiments performed upon the earth's surface. Fizeau, by a rapidly revolving toothed wheel, estimated the velocity at 195,920 miles per second. As this result differed by about one part in sixty from estimates previously accepted, there was thought to be room for further investigation. The revolving mirror, previously used by Mr. Wheatstone in measuring the velocity of electricity, was now applied in a more refined manner by Fizeau and by Foucault to determine the velocity of light. The latter physicist finally came to the startling conclusion that the velocity was not really more than 185,172 miles per second. No repetition of the experiment as thus performed would shake this result, and there was accordingly a discrepancy between the two astronomical and the experimental results of about 7000 miles per second demanding explanation.

Now a very little consideration shows that both the astronomical determinations involve the magnitude of the earth's orbit as one datum, because our estimate of the earth's velocity in its orbit depends upon our estimate of the sun's mean distance. Accordingly as regards this quantity the two astronomical results must count only for one. Though the transit of Venus had been considered to give the best data for the calculation of the sun's parallax and distance, yet astronomers had not neglected other less favourable opportunities. Thus Hansen, calculating from certain inequalities in the moon's motion, had estimated it at 8″·916; Winneke, from observations of Mars, at 8″·964; Leverrier, from the motions of Mars, Venus, and the moon, at 8″·950. Now these independent results agree much better with each other than with that of Bessel (8″·578) previously received, or that of Encke (8″·58) deduced from the transits of Venus in 1761 and 1769, and though each separately might be worthy of less credit, yet their close accordance renders their mean result (8″·943) probably comparable in probability with that of Bessel. It was further found that if Foucault's value for the velocity of light were assumed to be correct, and the sun's distance were inversely calculated from that and the other requisite data, the sun's parallax would appear to be 8″·960, which closely agreed with the above mean result. This further correspondence of independent results threw the balance of probability strongly against the results of the transit of Venus, and rendered it desirable to reconsider the observations made on that occasion.

Mr. E. J. Stone having re-discussed those observations[d] found that grave oversights had been made in the calculations, which being corrected would alter the estimate of parallax to 8″·91, a quantity in such comparatively close accordance with the other results that astronomers did not hesitate at once to reduce their estimate of the sun's mean distance from 95,274,000 to 91,771,000 miles, although this alteration involved a corresponding correction in the assumed magnitudes and distances of most of the heavenly bodies. The final decision of this question of the ratio between the earth and the visible universe, so far as it can be decided in the present century, must be made at the approaching transits of Venus in 1874 and 1882.

In this important and interesting question the theoretical relations between the velocity of light, the constant of aberration, the sun's parallax, and the sun's mean distance, are of the simplest character, and can hardly be open to any doubt, so that the only doubt was as to which result of observation was the most reliable. Eventually the chief discrepancy was found to arise from misapprehension in the reduction of observations, but we have a satisfactory example of the value of different methods of estimation in leading to the detection of a serious error. Is it not surprising that Foucault by measuring the velocity of light when passing through the space of a few yards, should lead the way to a change in our estimates of the magnitude of the whole universe?

Selection of the best Mode of Measurement.

When we have once obtained a command over a question of physical science by comprehending the theory of the subject, we have often a wide choice opened to us as regards the methods of measurement, which may thenceforth be made to give the most accurate results. If we can only measure one fundamental quantity we may often be able by correct theory to assign with accuracy a great many other quantitative results. Thus, if we can once determine satisfactorily the atomic weights of certain elements, we do not need to determine with equal accuracy the composition and atomic weights of their several compounds. When we have once learnt the relative atomic weights of oxygen and sulphur we can calculate the composition by weight of the several oxides of sulphur. Chemists accordingly select with the greatest care that compound of any two elements which seems to allow of the most accurate analysis so as to give the ratio of their atomic weights. It is obvious that we only need to have the ratio of the atomic weight of each element to that of some other common element, in order to calculate with the greatest ease that of each to each. Moreover the atomic weight stands in simple relation to other quantitative facts. The weights of equal volumes of elementary gases at equal temperature and pressure have the same ratio as the atomic weights; now as nitrogen weighs 14·06 times as much as hydrogen, under such circumstances we may infer that the atomic weight of nitrogen is about 14·06 (probably 14·00) that of hydrogen being unity. There is much evidence, again, to show that the specific heats of elements, and even of compounds, are inversely as their atomic weights, so that these two classes of quantitative data throw light mutually upon each other. In fact the atomic weight, the atomic volume, and the atomic heat of an element, are quantities so closely connected that the determination of any one may lead to that of the others. The chemist accordingly has to solve a most complicated problem in deciding in the case of each of 60 or

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[d] 'Monthly Notices of the Royal Astronomical Society,' vol. xxviii. p. 264.

70 elements which mode of determination is most accurate. Modern chemistry presents us with an almost infinitely extensive web of numerical ratios developed out of a comparatively few fundamental ratios.

In hygrometry we are presented with a choice among at least four modes of measuring the quantity of aqueous vapour contained in a given bulk of air. We can extract the vapour by absorption in sulphuric acid, and directly weigh its amount; we can place the air in a barometer tube and observe how much the absorption of the vapour alters the elastic force of the air; we can observe the dew point of the air, or the temperature at which the vapour becomes saturated; or, lastly, we can insert a dry and wet bulb thermometer and observe the temperature of an evaporating surface. Now the results of each such mode can be connected by well-established theory with those of the other modes, and we can select for each experiment that mode which is either most accurate or most convenient. The chemical method of direct measurement is probably capable of the greatest accuracy, but is troublesome; the dry and wet bulb thermometer is sufficiently exact for meteorological purposes.

Agreement of Distinct Modes of Measurement.

Many illustrations might be given of the accordance which has been found to exist in some cases between the results of entirely different methods of arriving at the measurement of a physical quantity. While such accordance must, in the absence of any information to the contrary, be regarded as the best possible proof of the approximate correctness of the mean result, yet instances have occurred to show that we can never take too much trouble in confirming experimental results of great importance. Even when three or more distinct methods have given nearly coincident results, a new method has sometimes disclosed a discrepancy which it is yet impossible to explain.

The ellipticity of the earth is known with very considerable approach to certainty and accuracy, for it has been estimated in three independent ways. The most direct mode is to measure long arcs extending north and south upon the earth's surface, by means of trigonometrical surveys, and then to compare the lengths of these arcs with the amount of their curvature as determined by the observation of the altitude of certain stars at the terminal points. The most probable ellipticity of the earth deduced from all measurements of this kind was estimated by Bessel at $\frac{1}{300}$, though subsequent measurements might lead to a slightly different estimate. The divergence from a globular form causes a small variation in the force of gravity in different parts of the earth's surface, so that exact pendulum observations give the data for an entirely independent estimate of the ellipticity, which is thus found to be $\frac{1}{320}$. In the third place the spheroidal protuberance about the earth's equator leads to a certain inequality in the moon's motion, as shown by Laplace; and from the amount of that inequality, as given by observations, Laplace was enabled to calculate back to the amount of its cause. He thus inferred that the ellipticity is $\frac{1}{305}$, which lies between the two numbers previously given, and was considered by him to be the most satisfactory conclusion. In this case the accordance is both close and undisturbed by any other or subsequent results, so that we are obliged to accept Laplace's result as a highly probable and accurate one.

The mean density of the earth is another constant quantity of the highest importance, because it forms the starting-point for the determination of the masses of other heavenly bodies. Physicists have accordingly bestowed a great amount of labour upon the exact estimation of this density, consisting in the exact comparison of the gravity of the whole globe with the gravity of some selected body of matter, of which the mass, or what comes to the same thing, the density compared with water, is known more or less exactly. But this body of matter may be variously chosen; it may consist of a heavy ball of lead, or a mountain, or a portion of the earth's strata, and the methods of experiment are so very different in these different cases that they may be regarded as giving entirely independent results.

The mutual gravitation of two balls, or other small objects at the earth's surface, is so exceedingly small compared with their gravitation towards the immense mass of the earth, that it is usually quite imperceptible, and although asserted by Newton to exist, on the ground of theory, was never detected until the end of the 18th century. Michell attached two small balls to the extremities of a delicately suspended torsion balance, and then bringing heavy balls of lead alternately to each side of these small balls was able to detect a certain slight deflection of the torsion balance, which was a new verification of the theory of gravitation. Cavendish carried out the experiment with more care, and by estimating the actual gravitation of the balls by treating the torsion balance as a pendulum, and then taking into account the respective distances of the balls from each other and from the centre of the earth, was able to assign 5·48 (or as recomputed by Baily, 5·448) as the probable mean density of the earth. Newton's sagacious guess to the effect that the density of the earth was between five and six times that of water, was thus remarkably confirmed. The same kind of experiment repeated by Reich gave 5·438. Baily having again performed the experiment with every possible refinement obtained a slightly higher number, 5·660.

A different method of procedure consisted in ascertaining the effect of a mountain mass in deflecting the plumb-line; for assuming that we can determine the dimensions and mean density of the mountain the plumb-line enables us to compare its mass with that of the whole earth. The Mountain Schehallien was selected for such an experiment, and the observations and calculations performed by Maskelyne, Hutton, and Playfair, gave as the most probable result, 4·713. The difference is considerable and the result is valuable, because the instrumental operations are of an entirely different character from those of Cavendish and Baily's experiments. Sir Henry James' similar determination from the attraction of Arthur's Seat gave 5·14.

A third distinct method consists in determining the force of gravity at points elevated above the surface of the earth on mountain ranges, or sunk below it in mines. Carlini experimented with a pendulum at the hospice of Mont Cenis, 6375 feet above the sea, and by comparing the attractive forces of the earth and the mountains, found the density to be still smaller, namely, 4·39, or as corrected by Giulio, 4·950. Lastly, the Astronomer Royal has on two occasions adopted the opposite method of observing a pendulum at the bottom of a deep mine, so as to compare the density of the strata penetrated with the density of the whole earth. On the second occasion he carried his method into effect at the Harton Colliery, 1260 feet deep; all that could be accomplished by skill in measurement and careful consideration of all the causes of error, was accomplished in this elaborate series of observations[c] (see vol. i. p. 340). No doubt Sir George Airy was much surprised and perplexed when he found that his new result considerably exceeded that obtained by any other method, being no less than 6·566, or 6·623 as finally corrected.

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[c] 'Philosophical Transactions' (1856), vol cxlvi. p. 342.

In 1844 Sir John Herschel remarked in his Memoir of Francis Baily[f], that 'the mean specific gravity of this our planet is, in all human probability, quite as well determined as that of an ordinary hand-specimen in a mineralogical cabinet,—a marvellous result, which should teach us to despair of nothing which lies within the compass of number, weight, and measure.' But at the same time he pointed out that Baily's final result, of which the probable error was only 0·0032, was the highest of all determinations then known, and Airy's investigation has since given a much higher result, quite beyond the limits of probable error of any of the previous experiments. If we treat all determinations yet made as of equal weight, the simple mean is about 5·45, the mean error nearly 0·5, and the probable error almost 0·2, so that it is as likely as not that the truth lies between 5·65 and 5·25 on this view of the matter. But it is remarkable that the two most recent and careful series of observations, by Baily and Airy[g], lie beyond these limits, and as with the increase of care the estimate rises, it seems requisite to reject the earlier results, and look upon the question as still requiring further investigation. In this case we learn an impressive lesson concerning the value of repeated determinations by distinct methods in disabusing our minds of the reliance which we are only too apt to place in results which show a certain degree of coincidence.

Since the establishment of the dynamical theory of heat it has become a matter of the greatest importance to determine with accuracy and high probability the mechanical equivalent of heat, or the quantity of energy which must be given, or received, in a definite change of temperature effected in a definite quantity of a standard substance, such as water. No less than seven almost entirely distinct modes of determining this constant have been tried. Dr. Joule first ascertained by the friction of water that to raise the temperature of one kilogram of water through one degree centigrade, we must employ energy sufficient to raise 424 kilograms through the height of one metre against the force of gravity at the earth's surface. Joule, Mayer, Clausius[h], Favre and other experimentalists have made various other determinations by less direct methods, and their results may be thus summed up[i].

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[f] 'Monthly Notices of the Royal Astronomical Society' for 8th Nov. 1844, No. X, vol. vi. p. 89.

[g] 'Philosophical Magazine,' 2nd Series, vol. xxvi. p. 61.

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Considering the diverse and in many cases difficult methods of observation, these results exhibit satisfactory accordance, and their mean (423·9) comes very close to the number derived by Dr. Joule from the apparently most accurate method. The constant generally assumed as the most probable result is 423·55 kilogrammetres, or gramme metres, if the quantity of water heated 1° Cent. be one gramme instead of a kilogramme.

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[h] Clausius, 'Philosophical Magazine,' 4th Series, vol. ii. p. 119.

[i] Watts' 'Dictionary of Chemistry,' vol. iii. p. 129.

Residual Phenomena.

Even when all the experimental data employed in the verification of a theory are sufficiently accurate, and the theory itself is sound, there may still exist discrepancies demanding further investigation. Sir John Herschel was perhaps the first who pointed out the importance of such outstanding quantities, and called them *residual phenomena*[j]. Now if the observations and the theory be really correct, such discrepancies must be due to the incompleteness of our knowledge of the causes in action, and the ultimate explanation must consist in showing that there is in action

(1) Some agent of known nature whose presence was not suspected.

(2) Some new agent of unknown nature.

In the first case we cannot be said to make any new discovery, for our ultimate success consists merely in reconciling the theory with known facts when our investigation is more comprehensive. But in the second case we meet with a totally new fact, which may lead us to whole realms of new discovery. Take the instance adduced by Sir John Herschel. The theory of Newton and Halley concerning cometary motions was that they were gravitating bodies revolving round the sun in oblique orbits, and the actual return of Halley's Comet, in 1758, sufficiently verified this theory. But, when accurate observations of Encke's Comet came to be made, the verification was not found to be complete. Each time Encke's Comet returned a little sooner than it ought, the period having regularly decreased from 1212·79 days, between 1786 and 1789, to 1210·44 between 1855 and 1858. The theory of gravitation alone cannot account for such a continued decrease of period; hence the hypothesis has been started that there is a resisting medium filling the space through which the comet passes. This hypothesis is a *deus ex machinâ* for explaining this solitary phenomenon, and cannot possess any validity or probability unless it can be shown that other phenomena are deducible from it. Many persons have identified this medium with that through which heat undulations pass, but I am not aware that there is anything in the undulatory theory of light to show that the medium would offer resistance to a moving body. If Professor Balfour Stewart can prove that a rotating disc experiences resistance even in a perfectly vacuous receiver, here is an experimental fact which distinctly supports the hypothesis. But in the mean time it is open to question whether other known agents, for instance electricity, may not be brought in, and I have tried to show that if, as seems highly probable, on other grounds, the tail of a comet is an electrical phenomenon, it is almost a necessary result of the theory of the conservation of energy that the comet shall exhibit a loss of energy manifested in a diminution of its mean distance from the sun and its period of revolution[k]. If so, the residual phenomenon seems to confirm an hypothesis as to the nature of the comet itself, rather than that of the medium through which it moves.

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[j] 'Preliminary Discourse on the study of Natural Philosophy,' §§ 158, 174. 'Outlines of Astronomy,' 4th. edit. § 856.

In other cases residual phenomena have involved important inferences not recognised at the time. Newton showed how the velocity of sound in the atmosphere could be calculated by a theory of pulses or undulations from the observed tension and density of the air. He inferred that the velocity in the ordinary state of the atmosphere at the earth's surface would be 968 feet per second, and very rude experiments made by him in the cloisters of Trinity College seemed to show that this was not far from the truth. Subsequently it was ascertained by other experimentalists that the velocity of sound was more nearly 1142 feet, and the discrepancy being no less than one sixth part of the whole was far too much to attribute to casual errors in the numerical data. Newton attempted to explain away this discrepancy by hypotheses as to the relations of the molecules of air, but without success.

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[k] 'Proceedings of the Manchester Literary and Philosophical Society,' 28th November 1871, vol. xi. p. 33.

Many new investigations having been made from time to time concerning the velocity of sound, both as observed experimentally and as calculated from theory, it was found that each of Newton's results was inaccurate, the theoretical velocity being 916 feet per second, and the real velocity about 1090 feet. The discrepancy therefore remained as serious as ever, and it was not until the year 1816 that Laplace showed it to be due to the heat developed by the sudden compression of the air in the passage of the wave, this heat having the effect of increasing the elasticity of the air and accelerating the motion of the impulse. It is now perceived that this discrepancy really involved the whole doctrine of the equivalence of heat and energy, and the discrepancy was applied by Mayer, at least by implication, to give an estimate of the mechanical equivalent of heat. The estimate thus derived agrees satisfactorily with independent and more direct determinations by Dr. Joule and other physicists, so that the explanation of the residual discrepancy which so exercised Newton's ingenuity is now complete.

As Sir John Herschel observed, almost all the great astronomical discoveries have been first disclosed in the form of residual differences. It is the practice at well-conducted observatories to compare the position of the principal heavenly bodies as actually observed with what might have been expected theoretically. This practice was introduced by Halley when Astronomer Royal, and his reduction of the lunar observations gave a series of residual errors from 1722 to 1739, by the examination of which the lunar theory was improved. Most of the greater astronomical variations arising from nutation, aberration, planetary perturbation were in like manner disclosed. The precession of the equinox was perhaps the earliest residual difference observed; the systematic divergence of Uranus from its calculated places was one of the latest, and was the foundation of the remarkable discovery of Neptune by anticipation. We may also class under residual phenomena all the so-called proper motions of the stars. A complete star catalogue, such as that of the British Association, gives a greater or less amount of proper motion for almost every star, consisting in the apparent difference of position of the star as derived from the earliest and latest good observations. But these apparent motions are often due, as is expressly explained by Baily[1], the author of the catalogue, to errors of observation and reduction. In many cases the best astronomical authorities have differed as to the very direction of the supposed proper motion of stars, and as regards the amount of the motion, for instance of α Polaris, the most different estimates have been formed. Residual quantities will of necessity be often so small that their very existence will be doubtful. Only the gradual progress both of theory and of accurate measurement will clearly show whether a discrepancy is to be referred to previous errors of observation and theory or to some new phenomenon. But nothing is more requisite for the progress of science than

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[1] 'British Association Catalogue of Stars,' p. 49.

CHAPTER XXVI.

CHARACTER OF THE EXPERIMENTALIST.

THERE seems to be a tendency to believe that, in the present age, the importance of individual genius is less than it formerly was.

> 'The individual withers, and the world is more and more.'

Society, it seems to be supposed, has now assumed so highly developed a form, that what was accomplished in past times by the solitary exertions of a single great intellect, may now be gradually worked out by the united labours of an army of investigators. Just as the combination of well-organized power in a modern army entirely supersedes the single-handed bravery of the mediæval knight, so we are to believe that the combination of intellectual labour has superseded the genius of an Archimedes, a Roger Bacon, or a Newton. So-called original research is now regarded almost as a recognised profession, adopted by hundreds of men, and communicated by a regular system of training. All that we need to secure great additions to our knowledge of nature is the erection of great laboratories, museums, and observatories, and the offering of sufficiently great pecuniary rewards to those who can invent new chemical compounds, or detect new species, or discover new comets. Doubtless this is not the real meaning of the eminent men who are now urging upon Government the elaborate endowment of physical research. They can only mean that the greater the pecuniary and material assistance given to men of science, the greater is the result which the available genius of the country may be expected to produce. Money and opportunities of study can no more produce genius than sunshine and moisture can generate living beings; the inexplicable germ is wanting in both cases. But, just as when the germ is present, the plant will grow more or less vigorously according to the circumstances in which it is placed, so it may be allowed that pecuniary assistance may favour the development of intellect. Public opinion however is not discriminating, and is likely to interpret the agitation for the endowment of science as meaning that science can be evolved from money or labour.

All such notions are, I believe, radically erroneous. In no branch of human affairs, neither in politics, war, literature, industry, nor science, is the influence of genius less considerable than it used to be. It is quite possible that the extension and organization of scientific study, assisted by the printing press and the accelerated means of communication, has increased the rapidity with which new discoveries are made known, and their details worked out by many heads and hands. A Darwin now no sooner propounds original ideas concerning the evolution of animated creatures, than those ideas are discussed and illustrated, and applied by other naturalists in every part of the civilized world. In former days his labours and discoveries would have been hidden for decades of years in scarce manuscripts, and generations would have passed away before his theory had enjoyed the same amount of criticism and corroboration as it has already received in fifteen years. But the general result is that the genius of Darwin is more valuable, not less valuable, than it would formerly have been. The advance of military science and the organization of enormous and well disciplined armies has not decreased the value of a skilful general; on the contrary, the rank and file are still more in need than they used to be of the guiding power of an ingenious and far-seeing intellect. The swift destruction of the French military power was not due alone to the perfection of the German army, nor to the genius of Moltke; it was due to the combination of a well-disciplined multitude, with a leader of the highest intellectual powers. So in every branch of human affairs the influence of the individual is not withering, but is growing with the extent of the material resources which are at his command.

Nature of Genius.

Turning to our own particular subject, it is a work of undiminished interest to reflect upon those qualities of mind which lead to great advances in natural knowledge. Nothing, indeed, is less amenable than genius to scientific analysis and explanation. Even precise definition is out of the question. Buffon said that 'genius is patience,' and certainly patience is one of its most constant and requisite components. But no one can suppose that patient labour alone will invariably lead to those conspicuous results which we attribute to genius. In every branch of science, literature, art, or industry, there are thousands of men and women who work with unceasing patience, and thereby ensure at least a moderate success; but it would be absurd to assent for a moment to crude notions of human equality, and to allow that equal amounts of intellectual labour yield equal results. A Newton may modestly and sincerely attribute his discoveries to industry and patient thought, and there is much reason to believe that genius is essentially unconscious and unable to account for its own peculiar powers.

If genius, indeed, be that by which intellect diverges from what is common, it must necessarily be a phenomenon beyond the domain of the ordinary laws of nature. Nevertheless, it is always an interesting and instructive work to trace out, as far as possible, the characteristics of mind by which great discoveries have been achieved, and we shall find in the analysis much to illustrate the principles of scientific method.

Error of the Baconian Method.

Hundreds of investigators may be constantly engaged in experimental inquiry; they may compile numberless notebooks full of scientific facts, and may frame endless tables full of numerical results; but if the views of the nature of induction here maintained be true they can never by such work alone rise to new and great discoveries. By an organized system of research they may work out deductively the detailed results of a previous discovery, but to arrive at a new principle of nature is another matter. Francis Bacon contributed to spread abroad the hurtful notion that to advance science we must begin by accumulating facts, and then draw from them, by a process of patient digestion, successive laws of higher and higher generality. In protesting against the false method of the scholastic logicians, he exaggerated a partially true philosophy, until it became almost as false as that which preceded it. His notion of scientific method was that of a kind of scientific bookkeeping. Facts were to be indiscriminately gathered from every source, and posted in a kind of ledger, from which would emerge in time a clear balance of truth. It is difficult to imagine a less likely way of arriving at great discoveries.

The greater the array of facts, the less is the probability that they will by any routine system of classification or research disclose the laws of nature they embody. Exhaustive classification in all possible orders is out of the question, because the possible orders are practically infinite in number. It is before the glance of the philosophic mind that facts must display their meaning, and fall into logical order. The natural philosopher must therefore have, in the first place, a mind of impressionable character, which is readily affected by the slightest exceptional phenomenon. His associating and identifying powers must be great, that is, a single strange fact must suggest to his mind whatever of like nature has previously come within his experience. His imagination must be active, and bring before his mind multitudes of relations in which the unexplained facts may possibly stand with regard to each other, or to more common facts. Sure and vigorous powers of deductive reasoning must then come into play, and enable him to infer what will happen under each supposed condition. Lastly, and above all, there must be the love of certainty leading him diligently and with perfect candour, to compare his speculations with the test of fact and experiment.

Freedom of Theorizing.

It would be a complete error to suppose that the great discoverer is one who seizes at once unerringly upon the truth, or has any special method of divining it. In all probability the errors of the great mind far exceed in number those of the less vigorous one. Fertility of imagination and abundance of guesses at truth are among the first requisites of discovery; but the erroneous guesses must almost of necessity be many times as numerous as those which prove well founded. The weakest analogies, the most whimsical notions, the most apparently absurd theories, may pass through the teeming brain, and no many of the thoughts and theories which have passed through the mind of a scientific investigator, have been crushed in silence and secresy by his own severe criticism and adverse examination; that in the most successful instances not a tenth of the suggestions, the hopes, the wishes, the preliminary conclusions have been realized.'

Nevertheless, in Faraday's researches published either in the 'Philosophical Transactions' or in minor papers, in his manuscript note-books, or in various other materials, fortunately made known in his interesting life by Dr. Bence Jones, we find invaluable lessons for the experimentalist. These writings are full of speculations which we must not judge by the light of subsequent discovery. It may even be said that Faraday sometimes committed to the printing press crude ideas which a cautious friend would have counselled him to keep back or suppress. There was occasionally even a wildness and vagueness in his notions, which in a less careful experimentalist might have been fatal to the attainment of truth. This is especially apparent in a curious paper concerning Ray-vibrations; but fortunately Faraday was fully aware of the shadowy character of his speculations, and expressed the feeling in words which must be quoted. 'I think it likely,' he says[a], 'that I have made many mistakes in the preceding pages, for even to myself my ideas on this point appear only as the shadow of a speculation, or as one of those impressions upon the mind, which are allowable for a time as guides to thought and research. He who labours in experimental inquiries knows how numerous these are, and how often their apparent fitness and beauty vanish before the progress and development of real natural truth.' If, then, the experimentalist has no royal road to the discovery of the truth, it is an interesting matter to consider by what logical procedure he attains the truth.

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[a] 'Experimental Researches in Chemistry and Physics,' p. 372. Philosophical Magazine, 3rd Series, May 1846, vol. xxviii. p. 350.

If I have taken a correct view of logical method, there is really no such thing as a distinct process of induction. The probability is infinitely small that a collection of complicated facts will fall into an arrangement capable of exhibiting directly the laws obeyed by them. The mathematician might as well expect to integrate his functions by a ballot-box, as the experimentalist to draw deep truths from haphazard trials. All induction is but the inverse application of deduction, and it is by the inexplicable mental action of a gifted mind that a multitude of heterogeneous facts are caused to range themselves in luminous order as the results of some uniformly acting law. So different, indeed, are the qualities of mind required in different branches of science that it would be absurd to attempt to give an exhaustive description of the character of mind which leads to discovery. The labours of Newton could not have been accomplished except by a mind of the utmost mathematical genius; Faraday, on the other hand, has made the most extensive and undoubted additions to human knowledge without ever passing beyond common arithmetic. I do not remember meeting in Faraday's writings with a single algebraic formula or mathematical problem of any complexity. Professor Clerk Maxwell, indeed, in the preface to his new 'Treatise on Electricity,' has strongly recommended the reading of Faraday's researches by all students of science, and has given his opinion that though Faraday seldom or never employed mathematical formulæ, his methods and conceptions were not the less mathematical in their nature[b]. I have myself protested against the prevailing confusion between a mathematical and an exact science[c], yet I certainly think that Faraday's experiments were for the most part purely qualitative, and that his mathematical ideas were of a rudimentary character. It is true that he could not possibly investigate such a subject as magne-crystallic action without involving himself in geometrical relations of considerable complexity. I nevertheless think that he was deficient in purely mathematical deductive power, that power which is so exclusively developed by the modern system of mathematical training at Cambridge. Faraday, for instance, was perfectly acquainted with the forms of his celebrated lines of force, but I am not aware that he ever entered into the subject of the algebraic nature of those curves, and I feel sure that he could not have explained their form as depending on the resultant attraction of all the magnetic particles acting according to general mathematical laws. There are even occasional indications that he did not understand some of the simpler mathematical doctrines of modern physical science. Although he so clearly foresaw the establishment of the unity of the physical forces, and laboured so hard with his own hands to connect gravity with the other forces, it is very doubtful whether he understood the fundamental doctrine of the conservation of energy as applied to gravitation. Thus, while Faraday was probably equal to Newton in experimental skill and deductive power as regards the invention of simple qualitative experiments, he was contrasted to him in mathematical power. These two instances are sufficient to show that minds of widely different conformation may meet with suitable regions of research. Nevertheless, there are certain common traits which we may discover in all the highest scientific minds.

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[b] See also 'Nature,' Sept. 18, 1873; vol. viii. p. 398.

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[c] 'Principles of Science,' vol. i. p. 317, and 'Theory of Political Economy,' pp. 3–14.

The Newtonian Method, the True Organum.

Laplace was of opinion that the 'Principia' and the 'Opticks' of Newton furnished the best models then available of the delicate art of experimental and theoretical investigation. In these, as he says, we meet with the most happy illustrations of the way in which, from a series of inductions, we may rise to the causes of phenomena, and thence descend again to all the resulting details.

The popular notion concerning Newton's discoveries is that in early life, while driven into the country by the Great Plague, a falling apple accidentally suggested to him the existence of gravitation, and that, availing himself of this hint, he was led to the discovery of the law of gravitation, the explanation of which constitutes the 'Principia.' It is difficult to imagine a more ludicrous and inadequate picture of Newton's labours and position. No originality, or at least priority, could be or was claimed by Newton as regards the discovery of the celebrated law of the inverse square, so closely associated with his name. In a well-known Scholium[d] he acknowledges that Sir Christopher Wren, Dr. Hooke, and Dr. Halley, had severally observed the accordance of Kepler's third law of motion of the planets with the principle of the inverse square.

Newton's work was really that of developing the methods of deductive reasoning and experimental verification, by which alone great hypotheses can be brought to the touch-stone of fact. Archimedes was the greatest of ancient philosophers, for he showed how mathematical theory could be wedded to physical experiments; and his works are the first true Organum. Newton is the modern

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[d] 'Principia,' bk. I. Prop. iv.

Archimedes, and the 'Principia' forms the true Novum Organum of scientific method. The laws which he actually established are great, but his example of the manner of establishing them is greater still. There is hardly a progressive branch of physical and mathematical science, excepting perhaps chemistry and electricity, which has not been developed from the germs of true scientific procedure which he disclosed in the 'Principia' or the 'Opticks.' Overcome by the success of his theory of universal gravitation, we are apt to forget that in his theory of sound he originated the mathematical investigation of waves and the mutual action of particles; that in his Corpuscular theory of light, however mistaken, he first ventured to apply mathematical considerations to molecular attractions and repulsions; that in his prismatic experiments he showed how far experimental verification could be pushed; that in his examination of the coloured rings named after him, he accomplished the most remarkable instance of minute measurement yet known, a mere practical application of which by M. Fizeau was recently deemed worthy of a medal by the Royal Society. We only learn by degrees how complete was his scientific insight; a few words in his third law of motion display his acquaintance with the fundamental principles of modern thermodynamics and the conservation of energy, while manuscripts long overlooked prove that in his inquiries concerning atmospheric refraction he had overcome the main difficulties of applying theory to one of the most complex of physical problems.

After all, it is only by examining the way in which he effected discoveries, that we can rightly appreciate his greatness. The 'Principia' treats not of gravity so much as of forces in general, and the methods of reasoning about them. He investigates not one hypothesis only, but mechanical hypotheses in general. Nothing so much strikes the reader of the work as the exhaustiveness of his treatment, and the almost infinite power of his insight. If he treats of central forces, it is not any one law of force which he discusses, but many, or almost all imaginable cases, the laws and results of each of which he sketches out in a few pregnant words. If his subject is a resisting medium, it is not air or water alone, nor any one resisting medium, but resisting media in general. We have a good example of his method in the Scholium to the twenty-second proposition of the second book, in which he runs rapidly over many possible suppositions as to the laws of the compressing forces which might conceivably act in an atmosphere of gas, a consequence being drawn from each case, and that one hypothesis ultimately selected which yields results agreeing with experiments upon the pressure and density of the terrestrial atmosphere.

Newton said that he did not frame hypotheses, but, in reality, the greater part of the 'Principia' is purely hypothetical, endless varieties of causes and laws being imagined which have no counterpart in nature. The most grotesque hypothéses of Kepler or Descartes were not more imaginary. But Newton's comprehension of logical method was perfect; no hypothesis was entertained unless it was definite in conditions, and admitted of unquestionable deductive reasoning; and the value of each hypothesis was entirely decided by the comparison of its consequences with facts. I do not entertain a doubt that the general course of his procedure is identical with that view of the nature of induction, as the inverse application of deduction, which I have advocated throughout these volumes. Francis Bacon held that science should be founded on experience, but he wholly mistook the true mode of using experience, and in attempting to apply his method he ludicrously failed. Newton did not less found his method on experience, but he seized the true method of treating it, and applied it with a power and success never since equalled. It is wholly a mistake to say that modern science is the result of the Baconian philosophy; it is the Newtonian philosophy and the Newtonian method which have led to all the great triumphs of physical science, and I repeat that the 'Principia' forms the true 'Novum Organum.'

In bringing his theories to a decisive experimental verification, Newton showed, as a general rule, an exquisite skill and ingenuity. In his hands a few simple pieces of apparatus were made to give results involving an unsuspected depth of meaning. His most beautiful experimental inquiry was that by which he proved the differing refrangibility of rays of light. To suppose that he originally discovered the power of a prism to break up a beam of white light would be a great mistake, for he speaks of procuring a glass prism to try the *celebrated phenomena of colours.* But we certainly owe to him the theory that white light is a mixture of rays differing in refrangibility, and that lights which differ in colour, differ also in refrangibility. Other persons might have conceived this theory; in fact, any person regarding refraction as a quantitative effect, must see that different parts of the spectrum have suffered different amounts of refraction. But the power of Newton is shown in the tenacity with which he followed his theory into every consequence, and tested each result by a simple but conclusive experiment. He first shows that different coloured spots are displaced by different amounts when viewed through a prism, and that their images come to a focus at different distances from the lense, as they should do, if the refrangibility differed. After excluding by various experiments a variety of indifferent circumstances, he fixes his attention upon the question whether the rays are merely shattered, disturbed, and spread out in a chance manner, as Grimaldi supposed, or whether there is a constant relation between the colour and the refrangibility. If Grimaldi was right, it might be expected that any part of the spectrum taken separately, and subjected to a second refraction, would suffer a new breaking up, and produce some new spectrum. Newton inferred from his own theory that a particular ray of the spectrum would have a constant refrangibility, so that a second prism would merely bend it more or less, but not further disperse it in any considerable degree. By simply cutting off most of the rays of the spectrum by a screen, and allowing the remaining narrow ray to fall on a second prism, he proved the truth of this conclusion; and then slowly turning the first prism, so as to vary the colour of the ray falling on the second one, he found that the spot of light formed by the twice-refracted ray travelled up and down, a palpable proof that the amount of refrangibility varied with the colour. For his further satisfaction, he sometimes refracted the light a third or fourth time, and he found that it might be refracted upwards or downwards or sideways, and yet for each coloured light there was a definite amount of refraction through each prism. He completes the proof by showing that the separated rays may again be gathered together into white light by an inverted prism. So that no number of refractions alters the character of the light. The conclusion thus obtained serves to explain the confusion arising in the use of a common lense; with homogeneous light he shows that there is one distinct focus, with mixed light an infinite number of foci, which prevent a clear view from being obtained at any one point.

What astonishes the reader of the 'Opticks' is the persistence with which Newton follows out the consequences of a preconceived theory, and tests the one notion by a wonderful variety of simple comparisons with fact.

It is certainly the theory which leads him to the experiments, and most of these could hardly be devised by accident. The fertility with which he invents new combinations, and foresees the results, subsequently verified, produces an invincible conviction in the reader that he has possession of the truth. Newton actually remarks that it was by mathematically determining all kinds of phenomena of colours which could be produced by refraction that he had 'invented' almost all the experiments in the book, and he promises that others who shall 'argue truly,' and try the experiments with care, will not be disappointed in the results.

The philosophic method of Huyghens was almost exactly the same as that of Newton, and Huyghens' investigation of the laws of double refraction furnishes almost equally beautiful instances of theory guiding experiment. Double refraction was first discovered by accident, so far as we know, and was described by Erasmus Bartholinus in 1669. The phenomenon then appeared to be entirely exceptional, and the laws governing the two separate paths of the refracted rays were so unapparent and complicated, that even Newton altogether misunderstood the phenomenon, and it was only at the latter end of the last century that scientific men generally began to comprehend its laws.

Nevertheless, Huyghens had, with rare genius, arrived at the true theory as early as 1678. He regarded light as an undulatory motion of some medium, and in his 'Traité de la Lumière,' he pointed out that, in ordinary refraction, the velocity of propagation of the wave is equal in all directions, so that the front of an advancing wave is spherical, and reaches equal distances in equal times. But in crystals, as he supposed, the medium would be of unequal elasticity in different directions, so that a disturbance would reach unequal distances in equal times, and the wave produced would have a spheroidal form. Huyghens was not satisfied with an unverified theory. He calculated what might be expected to happen when a crystal of calc-spar was cut in various directions, and he says, 'I have examined in detail the properties of the extraordinary refraction of this crystal, to see if each phenomenon which is deduced from theory would agree with what is really observed. And this being so, it is no slight proof of the truth of our suppositions and principles; but what I am going to add here confirms them still more wonderfully; that is, the different modes of cutting this crystal, in which the surfaces produced give rise to refraction exactly such as they ought to be, and as I had foreseen them, according to the preceding theory.'

The supremacy of Newton's mistaken corpuscular doctrine of light caused the theories and experiments of Huyghens to be disregarded for more than a century; but it is not easy to imagine a more beautiful or successful application of the true method of inductive investigation, theory guiding experiment, and yet wholly relying on experiment for confirmation.

Candour and Courage of the Philosophic Mind.

Perfect readiness to reject a theory inconsistent with fact is, then, a primary requisite of the philosophical mind. But it would be a mistake to suppose that this candour has anything akin to fickleness; on the contrary, readiness to reject a false theory may be combined with a peculiar pertinacity and courage in maintaining an hypothesis as long as its falsity is not actually apparent. There must, indeed, be no prejudice or bias distorting the mind, and causing it to under-estimate or pass over the unwelcome results of experiment. There must be that scrupulous honesty and flexibility of mind, which assigns an adequate value to all evidence; indeed the more a man loves his theory, the more scrupulous should be his attention to its faults. Nothing is more common in life than to meet with some theorist, who, by long cogitation over a single theory, has allowed it to mould his mind, and render him incapable of receiving anything but as a contribution to the truth of his one theory. A narrow and intense course of thought may sometimes lead to great results, but the adoption of a wrong theory at the outset is in such a mind irretrievable. The man of one idea has but a single chance of truth. The fertile discoverer, on the contrary, chooses between many theories, and is never wedded to any one, unless impartial and repeated comparison has convinced him of its validity. He does not choose and then compare; but he compares time after time, and then chooses.

Having once deliberately chosen, the philosopher may rightly entertain his theory with the strongest love and fidelity. He will neglect no objection; for he may chance at any time to meet a fatal one; but he will bear in mind the inconsiderable powers of the human mind compared with the tasks it has to undertake. He will see that no theory can at first be reconciled with all possible objections, simply because there may be many interfering causes, or the very consequences of the theory may have a complexity which prolonged investigation by successive generations of men may not exhaust. If then, a theory exhibit a number of very striking coincidences with fact, it must not be thrown aside until at least one *conclusive discordance* is proved, regard being had to possible error in establishing that discordance. In science and philosophy something must be risked. He who quails at the least difficulty will never establish a new truth, and it was not unphilosophic in Leslie to remark concerning his own experimental investigations into the nature of heat—

'In the course of investigation, I have found myself compelled to relinquish some preconceived notions; but I have not abandoned them hastily, nor, till after a warm and obstinate defence, I was driven from every post[e].'

Faraday's life, again, furnishes most interesting illustrations of this tenacity of the philosophical mind. Though so candid in rejecting some of his theories, there were others to which he clung through everything. One of his most favourite notions was finally realised in a brilliant discovery; another remains in doubt to the present day.

The Philosophic Character of Faraday.

In Faraday's researches concerning the connexion of magnetism and light, we find an excellent instance of the pertinacity with which a favourite theory may be held and pursued, so long as the results of experiment are simply nugatory and do not clearly negative the notions entertained. In purely quantitative questions, as we have seen, the absence of apparent effect can seldom be regarded as proving the absence of all effect. Now Faraday was convinced that some mutual relation must exist between magnetism and light. As early as 1822 he attempted to produce an effect upon a ray of polarized light, by passing it through water placed between the poles of a voltaic battery; but he was obliged to record that not the slightest effect was observable. During forty subsequent years the subject, we are told[f], rose again and again to his mind, and no failure could make him relinquish his search after this unknown relation. It was in the year 1845 that he gained the first success; on August 30th he began to work with common electricity, vainly trying glass, quartz, Iceland spar, &c. Several days of labour gave no result, yet he did not desist. Heavy glass, a transparent medium of great refractive powers, composed of borate of lead, was now tried, by being placed between the poles of a powerful electro-magnet, while a ray of polarized light was transmitted through it. When the poles of the electro-magnet were arranged in certain positions with regard to the substance under trial, no effects were apparent; but at last Faraday happened fortunately to place a piece of heavy glass so that contrary magnetic poles were on the same side, and now an effect was witnessed. The glass was found to have the power of twisting the plane of polarization of the ray of light.

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[e] 'Experimental Inquiry into the Nature of Heat.' Preface, p. xv.

[f] Bence Jones, 'Life of Faraday,' vol. i. p. 362.

All Faraday's recorded thoughts upon this great experiment are replete with curious interest. He attributes his success to the opinion, almost amounting to a conviction, that the various forms, under which the forces of matter are made manifest, have one common origin, and are so directly related and mutually dependent that they are convertible. 'This strong persuasion,' he says[g], 'extended to the powers of light, and led to many exertions having for their object the discovery of the direct relation of light and electricity. These ineffectual exertions could not remove my strong persuasion, and I have at last succeeded.' He describes the phenomenon in somewhat figurative language as *the magnetization of a ray of light*, and also as *the illumination of a magnetic curve or line of force*. He has no sooner got the effect in one case, than he proceeds, with his characteristic comprehensiveness of research, to test the existence of a like phenomenon in all the substances available. He finds that not only heavy glass, but solids and liquids, acids and alkalis, oils, water, alcohol, ether, all possess this power; but he was not able to detect its existence in any gaseous substance. His thoughts cannot be restrained from running into curious speculations as to the possible results of the power in certain cases. 'What effect,' he says, 'does this force have in the earth where the magnetic curves of the earth traverse its substance? Also what effect in a magnet?' And then he falls upon the wholly original notion that perhaps this force tends to make iron and oxide of iron transparent, a phenomenon never previously or since observed. We can meet with nothing more instructive as to the course of mind by which great discoveries are made, than these records of Faraday's patient labours, and his varied success and failure. Nor are his unsuccessful labours upon the relation of gravity and electricity less interesting, and worthy of study.

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[g] 'Life of Faraday,' vol. ii. p. 199.

Throughout a large part of his life, Faraday was possessed by the idea that gravity cannot be unconnected with the other forces of nature. On March 19th, 1849, he wrote in his laboratory book—'Gravity. Surely this force must be capable of an experimental relation to electricity, magnetism, and the other forces, so as to bind it up with them in reciprocal action and equivalent effect[h].' He filled twenty paragraphs or more with reflections and suggestions, as to the mode of approaching the subject by experiment. He anticipated that the approach of one body to another would develope electricity in them, or that a body falling through a conducting helix would excite a current changing in direction as the motion was reversed. '*All this is a dream*,' he remarks; 'still examine it by a few experiments. Nothing is too wonderful to be true, if it be consistent with the laws of nature;

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[h] See also his more formal statement in the 'Experimental Researches in Electricity,' 24th Series, § 2702, vol. iii. p. 161.

and in such things as these, experiment is the best test of such consistency.'

He executed many difficult and tedious experiments, which are described in the 24th Series of Experimental Researches; but the result was *nil.* And yet he concludes, 'Here end my trials for the present. The results are negative; they do not shake my strong feeling of the existence of a relation between gravity and electricity, though they give no proof that such a relation exists.'

He returned to the work when he was ten years older, and in 1858–9 recorded many remarkable reflections and experiments. He was much struck by the fact that electricity is essentially a *dual force,* and it had always been a peculiar conviction of Faraday that no body could be electrified positively without some other body becoming electrified negatively; some of his researches had been simple developments of this necessary relation. But observing that between two mutually gravitating bodies there was no apparent circumstance to determine which shall be positive and which negative, he does not hesitate to call in question an old opinion. 'The evolution of *one* electricity would be a new and very remarkable thing. The idea throws a doubt on the whole; but still try, for who knows what is possible in dealing with gravity.' We cannot but notice the candour with which he thus in his laboratory book acknowledges the doubtfulness of the whole thing, and is yet prepared as a forlorn hope to frame experiments in opposition to all his previous experience of the course of nature. For a time his thoughts flow on as if the strange detection were already made, and he had only to trace out its consequences throughout the universe. 'Let us encourage ourselves by a little more imagination prior to experiment,' he says, and then h [illegible] upon the infinity of actions in nature, in which the mutual relations of electricity and gravity would come into play; he pictures to himself the planets and the comets charging themselves as they approach the sun; cascades, rain, rising vapour, circulating currents of the atmosphere, the fumes of a volcano, the smoke in a chimney become so many electrical machines. A multitude of events and changes in the atmosphere seem to be at once elucidated by such actions; for a moment his reveries have the vividness of fact. 'I think we have been dull and blind not to have suspected some such results,' and he sums up rapidly the consequences of his great but imaginary theory; an entirely new mode of exciting heat or electricity, an entirely new relation of the natural forces, an analysis of gravitation, and a justification of the conservation of force. Such were Faraday's fondest dreams of what might be, and to many another philosopher they would have been a sufficient basis for the writing of a great book. But Faraday's imagination was within his full control; as he himself says, 'Let the imagination go, guarding it by judgment and principle, and holding it in and directing it by experiment.' His dreams soon took a very practical form, and for many subsequent days he laboured with ceaseless energy, on the staircase of the Royal Institution, in the clock tower of the Houses of Parliament, or in the Shot Tower at Southwark, raising and lowering heavy weights, and combining electrical helices and wires in every conceivable way. His skill and long experience in experiment were severely taxed to eliminate the effects of the earth's magnetism, and time after time he saved himself from accepting mistaken indications, which to another man might have seemed conclusive verifications of his theory. When all was done there remained absolutely no results. 'The experiments,' he says, 'were well made, but the results are *negative*;' and yet he adds, 'I cannot accept them as conclusive.' In this position the question remains to the present day; it may be that the effect was too slight to be detected, or it may be that the arrangments adopted were not suited to develope the particular relation which exists, just as Oersted could not detect electro-magnetism, so long as his wire was perpendicular to the plane of motion of his needle. But these are not matters which concern us further here. We have only to notice the profound conviction in the unity of natural laws, the active powers of inference and imagination, the unbounded licence of theorizing, combined above all with the utmost diligence in experimental verification which this remarkable research manifests.

Reservation of Judgment.

There is yet another characteristic needed in the philosophic mind; it is that of suspending judgment when the data are insufficient. Many people will express a confident opinion on almost any question which is put before them, but they thereby manifest not strength, but weakness and narrowness of mind. To see all sides of a complicated subject, and to weigh all the different facts and probabilities correctly, may require no ordinary powers of comprehension. Hence it is most frequently the philosophic mind which is in doubt, and the ignorant mind which is ready with a positive decision. Faraday has himself said, in a very interesting lecture[1], 'Occasionally and frequently the exercise of the judgment ought to end in *absolute reservation.* It may be very distasteful, and great fatigue, to suspend a conclusion; but as we are not infallible, so we ought to be cautious; we shall eventually find our advantage, for the man who rests in his position is not so far from right as he who, proceeding in a wrong direction, is ever increasing his distance.'

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[1] Printed in 'Modern Culture,' edited by Youmans, p. 219.

Arago presented a conspicuous example of this high quality of mind, as Faraday remarks; for when he made known his curious discovery of the relation of a magnetic needle to a revolving copper plate, a number of supposed men of science in different countries gave immediate and confident explanations of it, which were all wrong. But Arago, who had both discovered the phenomenon and personally investigated its conditions, declined to put forward publicly any theory at all.

At the same time we must not suppose that the truly philosophic mind can tolerate a state of doubt, while a chance of decision remains open. In science nothing like compromise is possible, and truth must be one. Hence, doubt is the confession of ignorance, and must involve a painful feeling of incapacity. But doubt lies between error and truth, so that if we choose wrongly we are further away than ever from our goal.

Summing up, then, it would seem as if the mind of the great discoverer must combine almost contradictory attributes. He must be fertile in theories and hypotheses, and yet full of facts and precise results of experience. He must entertain the feeblest analogies, and the merest guesses at truth, and yet he must hold them as worthless till they are verified in experiment. When there are any grounds of probability he must hold tenaciously to an old opinion, and yet he must be prepared at any moment to relinquish it when a single clearly contradictory fact is encountered. 'The philosopher,' says Faraday[k], 'should be a man willing to listen to every suggestion, but determined to judge for himself. He should not be biassed by appearances; have no favourite hypothesis; be of no school; and in doctrine have no master. He should not be a respecter of persons, but of things. Truth should be his primary object. If to these qualities be added industry, he may indeed hope to walk within the veil of the temple of nature.'.

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[k] Bence Jones, 'Life of Faraday,' vol. i. p. 225.

BOOK V.

GENERALIZATION, ANALOGY, AND CLASSIFICATION.

CHAPTER XXVII.

GENERALIZATION.

I HAVE endeavoured to show in preceding chapters that all inductive reasoning is an inverse application of deductive reasoning, and consists in demonstrating that the consequences of certain assumed propositions or laws agree with facts of nature gathered by active or passive observation. The fundamental process of reasoning, as stated in the outset, consists in inferring of any thing what we know of similar objects, and it is on this principle that the whole of deductive reasoning, whether simply logical or mathematico-logical, is founded. All inductive reasoning must therefore be founded on the same principle. Now it might seem that by a very plain use of this principle we might avoid the complicated processes of induction and deduction, and argue directly from one particular case to another, as the late Mr. J. S. Mill proposed. If the Earth, Venus, Mars, Jupiter, and other planets move in elliptic orbits, cannot we dispense with all elaborate precautions, and assert that Neptune, Ceres, or the last discovered planet must do so likewise? Do we not know that Mr. Gladstone must die, because he is like other men? May we not argue that because some men die therefore he must? Is it requisite to ascend by induction to the general proposition 'all men must die,' and then descend by deduction from that general proposition to the case of Mr. Gladstone? My answer will be undoubtedly that it is necessary to ascend to general propositions. The fundamental principle of the substitution of similars gives us no warrant in affirming of Mr. Gladstone what we know of other men, simply because we cannot be sure that Mr. Gladstone is exactly similar to other men. Until his death we cannot be perfectly sure that he possesses precisely all the attributes of other men; it is a question of probability, and I have endeavoured to explain the mode in which the theory of probability is applied to calculate the probability that from a series of similar events we may infer the recurrence of like events under identical circumstances. There is then no such process as that of inferring from particulars to particulars. A careful analysis of the conditions under which such an inference appears to be made, shows that the process is really a general one, and that what is inferred of a particular case might be inferred of all similar cases. All reasoning is essentially general, and all science implies generalization. In the very birth-time of philosophy this was held to be so: 'Nulla scientia est de individiis, sed de solis universalibus,' was the doctrine of Plato, delivered by Porphyry. And Aristotle[a] held a like opinion—*Οὐδεμία δὲ τέχνη σκοπεῖ τὸ καθ' ἕκαστον . . . τὸ δὲ καθ' ἕκαστον ἄπειρον, καὶ οὐκ ἐπιστητόν.* 'No art treats of particular cases; for particulars are infinite and cannot be known.' No one who holds the doctrine that reasoning may be from particulars to particulars, can be supposed to have the most rudimentary notion of what constitutes reasoning and science.

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[a] Aristotle's 'Rhetoric,' Liber I. 2. 11.

At the same time there can be no doubt that practically what we find to be true of many similar objects will probably be true of the next similar object. This is the result to which an analysis of the Inverse Method of Probabilities leads us, and, in the absence of any precise data from which we may calculate probabilities, we are usually obliged to make a rough assumption that similars in some respects are similars in other respects. Thus it comes to pass that a very large part of the reasoning processes in which scientific men are engaged, seems to consist in detecting similarities between objects, and then rudely assuming that the like similarities will be detected in other cases.

Distinction of Generalization and Analogy.

There is no distinction but that of degree between what is known as reasoning by *generalization* and reasoning by *analogy*. In both cases from certain observed resemblances we infer, with more or less probability, the existence of other resemblances. In generalization the resemblances have great extension and usually little intension, whereas in analogy we rely upon the great intension, the extension being of small amount (vol. i. p. 31). If we find that the qualities A and B are associated together in a great many instances, and have never been found separate, it is highly probable that on the next occasion when we meet with A, B will also be found to be present, and *vice versâ*. Thus wherever we meet with an object possessing gravity, it is found to possess inertia also, nor have we met with any material objects possessing inertia without discovering that they also possess gravity. The probability has therefore become very great, as indicated by the rules founded on the Inverse Method of Probabilities (vol. i. pp. 276–312), that whenever in the future we meet an object possessing either one of the properties of gravity and inertia, it will be found on examination to possess the other of these properties. This is a clear instance of the employment of generalization.

In analogy, on the other hand, we reason from likeness in many points to likeness in other points. The qualities or points of resemblance are now numerous, not the objects. At the poles of Mars are two white spots which resemble in many respects the white regions of ice and snow at the poles of the earth. There probably exist no other similar objects with which to compare these, yet the exactness of the resemblance enables us to infer, with high probability, that the spots on Mars would be found to consist of ice and snow, if we could examine them.

In short, many points of resemblance imply many more. From the appearance and behaviour of those white spots we infer that they have all the chemical and physical properties of frozen water. The inference is of course only probable, and based upon the improbability that aggregates of many qualities should be formed in a like manner in two or more cases, without being due to some single uniform condition or cause. In reasoning by analogy, then, we observe that two objects A B C D E ..... and A′ B′ C′ D′ E′ ..... have many like qualities, as indicated by the identity of the letters, and we infer that, since the first has another quality, X, we shall also discover this quality in the second case by sufficiently close examination. As Laplace says,—'Analogy is founded on the probability that similar things have causes of the same kind, and p ame effects. The more perfect this similari this probability'[b]. The nature

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ilités,' p. 86.

of analogical inference is also very correctly described in the Logic attributed to Kant, where the rule of ordinary induction is stated in the words '*Eines in vielen, also in allen,*' one quality in many things, therefore in all; and the rule of analogy is '*Vieles in einem, also auch das übrige in demselben*'[c], many (qualities) in one, therefore also the remainder in the same.

It is evident that there may be intermediate cases in which, from the resemblance of a moderate number of objects in several properties, we may infer to other objects. Probability must rest either upon the number of instances or the depth of resemblance, or upon the occurrence of both in sufficient degrees. What there is wanting in extension must be made up by intension, and *vice versâ*.

Two Meanings of Generalization.

The term generalization, as commonly used, includes two processes which are of different character, but are often closely associated together. In the first place, we generalize whenever we recognise even in two facts or objects a certain common nature. We cannot detect the slightest similarity without opening the way to inference from one case to the other. If we compare a cubical with a regular octahedral crystal, there is little apparent similarity; but, so soon as we perceive that either can be produced by the symmetrical modification of the other, we discover a groundwork of similarity in the constitution of the crystals, which enables us to infer many things of one, because they are true of the other. Our knowledge of ozone took its rise from the time when the similarity of smell, attending electric sparks, strokes of lightning, and the slow combustion of phosphorus, was noticed by

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[c] Kant's 'Logik,' § 84, Königsberg, 1800, p. 207.

Schönbein. There was a time when the rainbow was an entirely inexplicable phenomenon, a portent, like a comet, and a cause of superstitious hopes and fears. But we find the true spirit of science in Roger Bacon, who desires us to consider the objects which present the same colours as the rainbow; he mentions hexagonal crystals from Ireland and India, but he bids us not suppose that the hexagonal form is essential, for similar colours may be detected in many other transparent stones. Drops of water scattered by the oar in the sun, the spray from a water-wheel, the dew-drops lying on the grass in the summer morning, all display a similar phenomenon[d]. No sooner have we grouped together these apparently diverse instances, than we have begun to generalize, and have acquired a power of applying to one instance what we can detect of others. Even when we do not apply the knowledge gained to new objects and phenomena, our comprehension of those already observed is vastly strengthened and deepened by thus learning to view them as particular cases of one more general property.

A second process, to which the name of generalization is equally given, consists in passing from a given fact or partial law to a multitude of unexamined cases, which we believe to be subject to the same conditions. Instead of merely recognising similarity as it is brought before us, we predict its existence before our senses can detect it, so that generalization of this kind endows us with a prophetic power of more or less probability. Having observed that many substances assume, like water and mercury, the three states of solid, liquid, and gas, and having assured ourselves by frequent trial that the greater the means we possess of heating or cooling, the more substances we can vapourize and freeze, we pass confidently in advance of fact, and assume that all substances are capable of these three forms. Such a generalization was accepted by men of the high intellect of Lavoisier[e] and Laplace[f] before many of the corroborative facts now in our possession were known. The reduction of a single comet beneath the sway of gravity was at once considered sufficient indication that all comets must obey the same power. Few persons doubted that the same great law extended over the whole heavens; certainly the fact that a few stars out of many millions make manifest the action of gravity, is now held to be sufficient evidence to establish the general extension of the laws of Newton over the sphere of the visible universe.

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[d] Whewell's 'Philosophy of the Inductive Sciences,' 2nd edit. vol. ii. p. 171, quoting the 'Opus Majus,' p. 473.

Value of Generalization.

It might seem that if we know particular facts, there can be little use in connecting them together by a general law. The particulars must be more full of useful information than an abstract general statement. If we know, for instance, the properties of an ellipse, a circle, a parabola, and hyperbola, what is the use of learning all these properties over again in the general theory of curves of the second degree? If we understand the phenomena of sound and light and water-waves separately, what is the need of erecting a general theory of waves, which, after all, is inapplicable to practice until resolved into particular cases again? But, in reality, we never do obtain an adequate knowledge of particulars until we regard them as cases of the general. Not only is there a singular delight in discovering the many in the one, and the one in the many, but there is a constant interchange of light and knowledge.

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[e] 'Chemistry,' translated by Kerr, 3rd edit. pp. 63, 77.

[f] 'System of the World,' ditto vol. i. p. 202.

Properties which are unapparent in the hyperbola may readily be discovered in the ellipse. Most of the complex relations which the old geometers discovered in the circle will be reproduced *mutatis mutandis* in the other conic sections. The undulatory theory of light might have been unknown at the present day, had not the theory of sound supplied hints by analogy. The study of light has made known many phenomena of interference and polarization, the existence of which had hardly been suspected in the case of sound, but which may now be sought out, and perhaps found to possess unexpected interest and importance. The careful study of water-waves shows how waves may alter in form and velocity with varying depth of water. Analogous changes may sometimes be detected in sound waves. Thus there is a mutual interchange of aid.

'Every study of a generalization or extension,' as De Morgan has well said[g], 'gives additional power over the particular form by which the generalization is suggested. Nobody who has ever returned to quadratic equations after the study of equations of all degrees, or who has done the like, will deny my assertion that οὐ βλέπει βλέπων may be predicated of any one who studies a branch or a case, without afterwards making it part of a larger whole. Accordingly it is always worth while to generalize, were it only to give power over the *particular*. This principle, of daily familiarity to the mathematician, is almost unknown to the logician.'

Comparative Generality of Physical Properties.

Much of the value of science depends upon the knowledge which we gradually acquire of the different degrees of generality of properties and phenomena of various kinds.

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[g] 'Syllabus of a propo [illegible] 34.

The very use of science consists in enabling us to act with confidence, because we can foresee the result. Now this foresight must rest upon the knowledge of the powers which will come into play. That knowledge, indeed, can never be certain, because it rests upon imperfect induction, and the most confident beliefs and predictions of the physicist may be falsified. Nevertheless, if we always estimate the probability of each belief according to the due teaching of the data, and bear in mind that probability when forming our anticipations, we shall ensure the minimum of disappointment. Even when he cannot exactly apply the theory of probabilities, the physicist may acquire the habit of making judgments in general agreement with its principles and results.

Such is the constitution of nature, that the physicist soon learns to distinguish those properties which have wide and uniform extension, from those which vary between case and case. Not only are certain laws distinctly laid down, with their extension carefully defined, but a scientific training gives a kind of tact in judging how far other laws are likely to apply under any particular circumstances. We learn by degrees that crystals exhibit phenomena depending upon the directions of the axes of elasticity, which we must not expect in uniform solids. Liquids, compared even with non-crystalline solids, exhibit laws of far less complexity and variety; and gases assume, in many respects, an aspect of nearly complete uniformity. To trace out the branches of science in which varying degrees of generality prevail, would be found to be an inquiry of great interest and importance; but want of space, if there were no other reason, would forbid me to attempt it, except in a very slight manner.

Gases, so far as they are really gaseous, not only have exactly the same properties in all directions of space, but one gas exactly resembles other gases in a great many qualities.

All gases expand by heat, according to the one same law, and by nearly the same amount; the specific heats of equivalent weights are equal, and the densities, though not the same, are exactly proportional to the atomic weights. All such gases obey the general law, that the volume multiplied by the pressure, and divided by the absolute temperature, is constant or nearly so. The laws of diffusion and transpiration are the same in all cases, and, generally speaking, all physical laws, as distinguished from chemical laws, which apply to one gas apply equally to all other gases. Even when gases differ in chemical or physical properties, the differences are minor in degree or number. Thus the differences of viscosity are far less marked than in the liquid and solid states. Nearly all gases, again, are colourless, the exceptions being chlorine, the vapours of iodine, bromine, and some other substances.

Only in one single point, so far as I am aware, do gases present distinguishing marks unknown, or nearly so, in the solid and liquid states. I mean as regards the light given off when incandescent. Each gas, when sufficiently heated, yields its own peculiar series of rays, arising from the free vibrations of the constituent parts of the molecules when pursuing separate paths. Hence the possibility of distinguishing gases by the spectroscope. But the molecules of solids and liquids appear to be continually in conflict with each other, so that only a confused *noise* of atoms is produced, instead of a definite series of luminous chords. At the same temperature, accordingly, all solids and liquids give off nearly the same rays when strongly heated, and we have in this case a single exception to the general rule of the greater generality of [illegible] gases.

Liquids are in[illegible]diate in character between gas[illegible]ble of possessing different elasticity in different directions, and thus denuded of the rich geometrical complexity of solids, they retain the variety of density, colour, degrees of transparency, great diversity in surface tension, viscosity, coefficients of expansion, compressibility, and many other properties which we observe in solids, but not for the most part in gases. Though our knowledge of the physical properties of liquids is thus much wanting in generality at present, there is ground to hope that by degrees laws connecting and explaining the varieties of character may be traced out. Liquids ought to be compared together, not at uniform temperatures, but at points of temperature similarly related to the points of fusion and ebullition.

Solids are in every way contrasted to gases. Each solid substance has its own peculiar density, hardness, compressibility, degree of transparency, tenacity, elasticity, power of conducting heat and electricity, magnetic properties, capability of producing frictional electricity, and so forth. Even different specimens of the same kind of substance will be widely different, according to the accidental treatment it has received. And not only has each substance its own specific properties, but, when crystallized, its own properties peculiar to each direction, regard being had to the axes of crystallization. The velocity of radiation, the rate of conduction of heat, the coefficients of expansibility and compressibility, the thermo-electric properties, all vary in different crystallographic directions.

It is highly probable that many apparent differences among liquids, and even among solids, will be resolved and explained, when we learn to regard them under exactly corresponding circumstances. The extreme generality of the properties of gases is really only true at an infinitely high temperature, when they are all equally remote from their condensing points. Now, it is found that if we compare liquids—for instance, different kinds of alcohols—not at equal temperatures, but at points equally distant from their respective boiling-points, the laws and coefficients of expansion are nearly equal. The vapour-tensions of liquids also are much more nearly equal, when thus compared at corresponding points, and the boiling-points themselves appear to be simply related to the chemical composition in many cases. No doubt the progress of investigation will often enable us to discover generality, where we at present only see variety and puzzling complexity.

In some cases substances exhibit the same physical properties in the liquid as in the solid state. Lead has a high refractive power, whether in solution, or in solid salts, crystallized, or vitreous. The magnetic power of iron is conspicuous, whatever be its chemical condition; indeed, the magnetic properties of substances, though varying with temperature, seem not to be greatly affected by physical changes. Colour, absorptive power for heat or light rays, and a few other properties are also often the same both in liquids and gases. Iodine and bromine possess a deep colour whenever they are chemically uncombined. Nevertheless, we can seldom argue safely from the properties of a substance in one condition to that in another condition. Ice is an insulator, water a conductor of electricity, and the same contrast exists in most other substances. The conducting power of a liquid for electricity increases with the temperature, while that of a solid decreases. By degrees we may learn to distinguish between those properties of matter which depend upon the intimate construction of the chemical molecule, and those which depend upon the contact, conflict, mutual attraction, or other relations of distinct molecules. The properties of a substance with respect to light seem generally to depend upon the molecule; thus, the power of certain substances to cause the plane of polarization of a ray of light to rotate, is exactly the same whatever be its degree of density, or the diluteness of the solution in which it is contained. Taken as a whole, the physical properties of substances and their quantitative laws, present a problem of infinite complexity, and centuries must elapse before any moderately complete generalizations on the subject become possible.

Uniform Properties of all Matter.

Some laws are held to be true of all matter in the universe absolutely, without exception, no instance to the contrary having ever been noticed. This is the case with the laws of motion, as laid down by Galileo and Newton. It is also conspicuously true of the law of universal gravitation. The rise of modern physical science may perhaps be considered as beginning at the time when Galileo showed, in opposition to the Aristotelians, that matter is equally affected by gravity, irrespective of its form, magnitude, or texture. All objects fall with equal rapidity, when disturbing causes, such as the resistance of the air, are removed or allowed for. That which was rudely demonstrated by Galileo from the leaning tower of Pisa, was proved by Newton to a high degree of approximation, in an experiment which has already been referred to (vol. ii. p. 55).

Newton formed two pendulums of as nearly as possible similar outward shape, by taking two equal round wooden boxes, and suspending them by equal threads, eleven feet long. The motion of each pendulum was therefore equally subject to the resistance of the air. He filled one box with wood, and in the centre of oscillation of the other placed an equal weight of gold. The pendulums were then equal in weight and in size; and, on setting them simultaneously in motion, Newton found that they vibrated for a great length of time with exactly equal vibrations. He tried the same experiment with silver, lead, glass, sand, common salt, water, and wheat, instead of gold, and ascertained that the rapidity of motion of his pendulum was exactly the same whatever was the kind of matter inside them[h]. He considered that a difference of a thousandth part would have been apparent. The reader must observe that the pendulums were made of equal weight only in order that they might suffer equal retardation from the air. The meaning of the experiment is that all the substances manifest exactly equal acceleration from the force of gravity, and that therefore the inertia or resistance of matter to force, which is the only independent measure of mass in our possession, is always proportional to gravity.

These experiments of Newton were considered conclusive up to very recent times, when certain discordances between the theory and observations of the movements of planets led Nicolai, in 1826, to suggest that the equal gravitation of different kinds of matter might not be absolutely exact. It is perfectly philosophical and desirable thus to call in question, from time to time, some of the best accepted laws. On this occasion Bessel carefully repeated the experiments of Newton with pendulums composed of ivory, glass, marble, quartz, meteoric stones, &c., but was unable to detect the least difference. This conclusion is also confirmed by the ultimate agreement of all the calculations of physical astronomy based upon it. Thus, whether the mass of Jupiter [illegible] from the motion of its own satellites, from [illegible] a small planets, Vesta, Juno, &c., or from the perturbation of Encke's Comet, the results are closely accordant, showing that precisely the same law of gravity applies to the most different bodies which we can observe. The gravity or weight of a body, again, appears to be entirely independent of its other physical conditions, being totally unaffected by any alteration in the temperature, density, electric or magnetic condition, or other physical properties of the substance.

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[h] 'Principia,' bk. III. Prop. [illegible]

One almost paradoxical result of the law of equal gravitation is the theorem of Torricelli, to the effect that all liquids of whatever density fall or flow with equal rapidity. If there be two equal cisterns respectively filled with mercury and water, the mercury, though thirteen times as heavy, would flow from an aperture neither more rapidly nor more slowly than the water, and the same would be true of ether, alcohol, or any other liquids, allowance being made for the resistance of the air, and the differing viscosities of the liquids.

In its exact equality and its perfect independence of every circumstance, except mass and distance, the force of gravity stands apart from all the other forces and phenomena of nature, and has not yet been brought into any relation with them except through the general principle of the conservation of energy. Magnetic attraction, as remarked by Newton, follows a wholly different law as depending upon the chemical quality and molecular structure of each particular substance.

We must remember that in saying 'all matter gravitates,' we exclude from the term matter the basis of light-undulations, which is almost infinitely more extensive in amount, and obeys in many other respects the laws of mechanics. This adamantine basis of undulations appears, so far as can be ascertained, to be perfectly uniform in its properties when existing in space unoccupied by matter.

Light and heat are conveyed by it with equal velocity in all directions, and in all parts of space so far as observation informs us. But the presence of gravitating matter modifies the density and mechanical properties of the so-called ether in a way which is yet quite unexplained.

Leaving gravity, it is somewhat difficult to discover other laws which are equally true of all matter. Boerhaave was considered to have established that all bodies expand by heat, but not only is the expansion very different in different substances, but we now know positive exceptions. Many liquids and a few solids contract by heat at certain temperatures. There are indeed other relations of heat to matter which seem to be universal and uniform; thus all substances begin to give off rays of heat or light at the same temperature, according to the law of Draper; and gases will not be an exception if sufficiently condensed, as in the experiments of Frankland. Grove considers it to be universally true that all bodies in combining produce heat; all solids, with the doubtful exception of sulphur and selenium, in becoming liquid, and all liquids in becoming gases, absorb a certain quantity of heat; but the quantities of heat absorbed vary with the chemical qualities of the matter. On the other hand, Carnot's Thermodynamic Law is held to be exactly true of all matter without distinction; it expresses the fact that the amount of mechanical energy which might be theoretically obtained from a certain amount of heat energy depends only upon the temperatures between which a substance is made to change, so that whether an engine be worked by water, air, alcohol, ammonia, or any other substance, the result would theoretically be the same, if the boiler and condenser were employed at similar temperatures.

Variable Properties of Matter.

I have enumerated some of the few properties of matter, which are manifested in exactly the same manner by all substances, whatever be their differences of chemical or physical constitution. But by far the greater number of qualities vary in degree; substances are more or less dense, more or less transparent, more or less compressible, more or less magnetic, and so on. One very common result of the progress of science is to show that qualities supposed to be entirely absent from many substances are present only in so low a degree of intensity that the means of detection were insufficient. Newton believed that most bodies were not affected by the magnet at all; Faraday and Tyndall have rendered it very doubtful whether any substance whatever is wholly non-magnetic, including under that term diamagnetic properties. We are rapidly learning to believe that there are no substances absolutely opaque, or non-conducting, non-electric, non-elastic, non-viscous, non-compressible, insoluble, infusible, or non-volatile. All tends to become a matter of degree, or sometimes of direction. There may be some substances oppositely affected to others, as ferro-magnetic substances are oppositely affected to diamagnetics, or as substances which contract by heat are opposed to those which expand; but the tendency is certainly for every affection of one kind of matter to be represented by something similar in other kinds. On this account one of Newton's rules of philosophizing seems quite to lose all validity; he said, 'Those qualities of bodies which are not capable of being heightened and remitted, and which are found in all bodies on which experiment can be made, must be considered as universal qualities of all bodies.' As far as I can see, the contrary is more probable, namely, that qualities variable in degree will be found in every substance in a greater or less degree.

It is highly remarkable that Newton, whose method of investigation was logically perfect, seemed incapable of generalizing and describing his own procedure. His celebrated 'Rules of reasoning in Philosophy,' described at the commencement of the third book of the 'Principia,' are of very questionable truth, and still more questionable value.

Extreme Instances of Properties.

Although, as we have seen, substances usually differ only in degree as regards their physical properties, great interest may attach to particular substances which manifest a property in a conspicuous and intense manner. Every branch of physical science has usually been developed from the attention forcibly drawn to some singular substance. Just as the loadstone disclosed magnetism and amber frictional electricity, so did Iceland spar point out the existence of double refraction, and sulphate of quinine the phenomenon of fluorescence. When one such startling instance has drawn the attention of the scientific world, numerous less remarkable cases of the phenomenon will soon be detected, and it will probably prove that the property in question is actually universal to all matter. Nevertheless, the extreme instances retain their interest, partly in a historical point of view, partly because they furnish the most convenient substances for experiment.

Francis Bacon was fully aware of the value of such examples, which he called *Ostensive Instances*
giving, Free or Predominant Instances. '
he says, 'which show the nature
naked, in an exalted condition, or

of power; freed from impediments, or at least by its strength predominating over and suppressing them[i].' He mentions quicksilver as an ostensive instance of weight or density, thinking it not much less dense than gold, and more remarkable than gold as joining density to liquidity. The magnet is mentioned as an ostensive instance of attraction[k]. It would not be very easy to distinguish clearly between these ostensive instances and those which he calls *Instantiae Monodicae*, or *Irregulares*, or *Heteroclitae*, under which he places whatever is extravagant in its properties or magnitude, or exhibits least similarity to other things, such as the sun and moon among the heavenly bodies, the elephant among animals, the letter *s* among letters, or the magnet among stones[l].

In optical science great use has been made of the high dispersive power of the transparent compounds of lead, that is, the power of giving a long spectrum (vol. i. p. 32). Dolland having noticed the peculiar dispersive power of lenses made of flint-glass employed them to produce an achromatic arrangement. The element strontium presents a contrast to lead in this respect, being characterized by a remarkably low dispersive power; but I am not aware that this property has yet been turned to account.

Compounds of lead have both a high dispersive and a high refractive index, and in the latter respect they proved very useful to Faraday. Having spent much labour in preparing various kinds of optical glass, Faraday happened to form a compound of lead, silica, and boracic acid, now known as *heavy glass*, which possessed an intensely high refracting power. Many years afterwards in attempting to discover the action of magnetism upon light he failed to detect any effect, as [illegible] already mentioned (vol. ii. p. 235), until he happened to test a piece of the heavy glass. The peculiar refractive power of this medium caused the magnetic strain to be apparent, and the rotation of the plane of polarization was discovered.

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[i] 'Novum Organum,' bk. II. Aphorism 24.

[k] Ibid. Aphorism 25.

[l] Ibid. Aphorism 28.

In almost every other part of physical science there is some substance of powers pre-eminent for the special purpose to which it is put. Rock-salt is invaluable for its extreme diathermancy or transparency to the least refrangible rays of the spectrum. Quartz is equally valuable for its transparency, as regards the ultra-violet or most refrangible rays. Diamond is the most highly refracting substance which is at the same time transparent; were it more abundant and easily worked it would be of great optical importance. Cinnabar is distinguished by possessing a power of rotating the plane of polarization of light, from 15 to 17 times that of quartz. In electric experiments copper is employed for its high conducting powers and exceedingly low magnetic properties; iron is of course essential for its enormous and almost anomalous magnetic powers; while bismuth holds a like place as regards its diamagnetic powers, and was of much importance in Tyndall's decisive researches upon the polar character of the diamagnetic force[m]. In regard to magnecrystallic action the mineral cyanite is highly remarkable, being so powerfully affected by the earth's magnetism, that when delicately suspended, it will assume a constant position with regard to the magnetic meridian, and may almost be used like the compass needle. Sodium is distinguished by its unique light-giving powers, which are so extreme that probably one half of the whole number of stars in the heavens have a yellow tinge in consequence.

[illegible] v remarkable that water, though the most fl [illegible] istinguished in almost every

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[illegible] (1856), vol. cxlvi. p. 246.

respect by the most marked qualities. Of all known substances water has the highest specific heat, being thus peculiarly fitted for the purpose of warming and cooling, to which it is often put. It rises by capillary attraction to a height more than twice that of any other liquid. In the state of ice it is nearly twice as dilatable by heat as any other known solid substance[n]. In proportion to its density it has a far higher surface tension than any other substance, being in fact surpassed in absolute tension only by mercury, and it would not be difficult to extend considerably the list of its remarkable and useful properties.

Under extreme instances we may include cases of remarkably low powers or qualities, equally with those of the opposite extreme. Such cases seem to correspond to what Bacon calls *Clandestine Instances*, which exhibit a given nature in the least intensity, and as it were in a rudimentary state[o]. They may often be important, he thinks, as allowing the detection of the cause of the property by difference. I may add that in some cases they may be of use in experiments. Thus hydrogen is at once the least dense of all known substances, and has the least atomic weight. Liquefied nitrous oxide has the lowest refractive index of all known fluids[p]. The compounds of strontium have the lowest dispersive powers on light. It will be obvious that a property of very low degree may prove as curious and valuable a phenomenon as a property of very high degree.

The Detection of Continuity.

We should always bear in mind that phenomena which are in reality of a closely similar or even identical nature, may present to the senses very different appearances. Without a careful analysis of the changes which take place, we may often be in danger of widely separating facts and processes, which are actually instances of the same law. Extreme difference of degree or magnitude is a very frequent cause of error. It is truly difficult at the first moment to recognise any similarity between the gradual rusting of a piece of iron, and the rapid combustion of a heap of straw. Yet Lavoisier's chemical theory was founded upon the close similarity of the oxydizing process in one case and the other. We have only indeed to divide the iron into excessively small particles to discover that it is really the more combustible of the two, so that it actually takes fire spontaneously and burns like tinder. It is the excessive slowness of the process in the case of a massive piece of iron which disguises its real character.

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[n] 'Philosophical Magazine,' 4th Series, January 1870, vol. xxxix. p. 2.

[o] 'Novum Organum,' bk. II. Aphorism 25.

[p] Faraday's 'Experimental Researches in Chemistry and Physics,' p. 93.

If Xenophon reports truly, Socrates was seriously misled by not making sufficient allowance for extreme differences of degree and quantity. He rejected the acute opinion of Anaxagoras that the sun is a fire, on the ground that we can look at a fire, but not at the sun, and that plants grow by sunshine while they are killed by fire. He also pointed out that a stone heated in a fire is not luminous, and soon cools, whereas the sun ever remains equally luminous and hot[q]. All such mistakes evidently arise from not perceiving that difference of quantity may be so extreme as to assume the appearance of difference of quality. It is the least creditable thing we know of Socrates, that when pointing out these supposed mistakes of earlier philosophers, he advised his followers not to study astronomy.

Masses of matter of very different size may always be expected to exhibit great apparent differences of conduct, arising simply from the very various intensity of the forces brought into play. Many persons have thought it requisite to imagine occult forces producing the suspension of the clouds, and there have even been absurd theories representing cloud particles as minute water-balloons buoyed up by the warm air within them. But we have only to take proper account of the enormous comparative resistance which the air opposes to the fall of minute particles, to see that all cloud particles are probably constantly falling through the air, but so slowly that there is no apparent effect. Mineral matter again is always regarded as inert and incapable of spontaneous movement. We are struck by astonishment on observing in a powerful microscope, that every kind of solid matter suspended in extremely minute particles in pure water, acquires an oscillatory movement, often so marked as to resemble dancing or skipping. I conceive that this movement is entirely due to the vast comparative intensity of chemical actions when exerted upon minute particles, the effect being 5000 or 10,000 greater in proportion to the mass than in fragments of an inch diameter (vol. ii. p. 9).

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[q] 'Memor[illegible]

[r] Whewell, 'History of Inductive Sciences,' [illegible]

Much that was formerly obscure in the science of electricity, arose from the extreme differences of intensity and quantity in which this form of energy manifests itself. Between the instantaneous and brilliant discharge of a thunder-cloud and the gentle continuous current produced by two pieces of metal and some dilute acid, there was no apparent analogy whatever. It was therefore a work of great importance when Faraday demonstrated the identity of the forces in action, showing that common frictional electricity would decompose water like that from the voltaic battery. The relation of the phenomena became plain when he succeeded in showing that it would require 800,000 discharges of his large Leyden battery to decompose one single grain of water. Lightning was now seen to be electricity of excessively high tension, but extremely small quantity, the difference being somewhat analogous to that between the force of one million gallons of water falling through one foot, and one gallon of water falling through one million feet. Faraday estimated that one grain of water acting on four grains of zinc, would yield electricity enough for a great thunderstorm.

It was long believed that electrical conductors and insulators belonged to two opposed classes of substances. Between the inconceivable rapidity with which the current passes through pure copper wire, and the apparently complete manner in which it is stopped by a thin partition of gutta-percha or gum-lac, there seemed to be no resemblance. Faraday, again, laboured successfully to show that these were but the extreme cases of a chain of substances varying in all degrees in their powers of conduction. Even the best conductors, such as pure copper or silver offer some resistance to the electric current. The other metals have considerably higher powers of resistance, and we pass gradually down through oxides and sulphides. The best insulators, on the other hand, allow of an atomic induction which is the necessary antecedent of conduction. Hence Faraday inferred that whether we can measure the effect or not, all substances discharge electricity more or less[r]. One consequence of this doctrine must be, that every discharge of electricity produces an induced current. In the case of the common galvanic current we can readily detect the induced current in any parallel wire or other neighbouring conductor, and can separate the opposite currents which arise at the moments when the original currents begin and end. But a discharge of high tension electricity like lightning, though it certainly occupies time and has a beginning and an end, yet lasts so minute a fraction of a second, that it would be hopeless to attempt to detect and separate the two opposite induced currents, which are nearly simultaneous and exactly neutralise each other. Thus an apparent failure of analogy is explained away, and we are furnished with another instance of a phenomenon incapable of observation and yet theoretically known to exist[s].

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[r] 'Experimental Researches in Electricity,' Series xii. vol. i. p. 420.

Perhaps the most extraordinary and fundamental case of the detection of unsuspected continuity is found in the discovery of Cagniard de la Tour and Professor Andrews, that the liquid and gaseous conditions of matter are only remote points in a continuous course of change. Nothing is at first sight more apparently distinct than the physical condition of water and aqueous vapour. At the boiling-point there is an entire breach of continuity, and the gas produced is subject to laws incomparably more simple than the liquid from which it arose. But Cagniard de la Tour showed that if we maintain a liquid under sufficient pressure its boiling point may be indefinitely raised, and yet the liquid will ultimately assume the gaseous condition with but a small increase of volume. Professor Andrews, recently following out a similar course of inquiry, has shown that liquid carbonic acid may, at a particular temperature (30°·92 C.), and under the pressure of 74° atmosphere, be at once in a state indistinguishable from that of liquid and gas. At higher pressures carbonic acid may be made to pass from a palpably liquid state to a truly gaseous state without any abrupt change whatever. The subject is one of some complexity, because as the pressure is greater the abruptness of the change from liquid to gas gradually decreases, and finally vanishes. As similar phenomena or an approximation to them have been observed in various other liquids, there is little doubt that we may make a wide generalization, and assert that, under adequate pressure, every liquid might be made to pass into a gas without any breach of continuity[t].

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[s] 'Life of Faraday,' vol ii. p. 7.

The liquid state, again, is considered by Professor Andrews to be but an intermediate step between the solid and gaseous conditions. There are various indications that the process of melting is not perfectly abrupt; and could the experiments be made under adequate pressures, it is believed that every solid could be made to pass by insensible degrees into the state of liquid, and subsequently into that of gas.

These discoveries appear to open the way to most important and fundamental generalizations, but it is probable that in many other cases phenomena now regarded as discrete may be shown to be different degrees of the same process. The late Professor Graham was of opinion that chemical affinity differed but in degree from the ordinary attraction which holds different particles of a body together. He found that sulphuric acid continued to evolve heat when mixed even with the fiftieth equivalent of water that is added to it, so that there seemed to be no distinct limit to chemical affinity. He concludes—'There is reason to believe that chemical affinity passes in its lowest degree into the attraction of aggregation'[u].

The atomic theory is well established, but its limits are not marked out. As Mr. Justice Grove suggests, we may by selecting sufficiently high multipliers express any combination or mixture whatever of elements in terms of their equivalent weights[x]. Sir W. Thompson has suggested that the power which vegetable fibre, oatmeal, and many other substances possess of attracting and condensing aqueous vapour is probably continuous, or, in fact, identical with capillary attraction, which is capable of interfering with the pressure of aqueous vapour and aiding its condensation[y]. There are many cases of so-called catalytic or surface action, such as the extraordinary power of animal charcoal for attracting organic matter, or of spongy platinum for condensing hydrogen, which can only be considered as exalted cases of a much more general power of attraction. The number of substances which are decomposed by light in a striking manner is very limited; but many other substances, such as vegetable colours, are affected by long exposure; on the principle of continuity we might well expect to find that all kinds of matter are more or less susceptible of change by the incidence of light rays[z]. It is the opinion of Mr. Justice Grove that wherever an electric current passes there is a tendency to decomposition, a strain on the molecules, which when sufficiently intense leads to disruption. Even a metallic conducting wire may be regarded as tending to decomposition. Davy was probably correct in describing electricity as chemical affinity acting on masses, or rather, as Grove suggests, creating a disturbance through a chain of particles[a]. Laplace went so far as to suggest that all chemical phenomena may be regarded as the results of the Newtonian law of attraction, applied to atoms of various mass and position; but the time is probably long distant when the progress of molecular philosophy and of mathematical methods will enable such a generalization to be verified or refuted.

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[t] 'Nature,' vol. ii. p. 278.

[u] 'Journal of the Chemical Society,' vol. viii. p. 51.

[x] 'Correlation of Physical Forces,' 3rd edit. p. 184.

The Law of Continuity.

Under the title Law of Continuity we may place many applications of the general principle of reasoning, that what is true of one case will be true of similar cases, and probably true of what are probably similar. Whenever we find that a law or similarity is rigorously fulfilled up to a certain point in time or space, we expect with a very high degree of probability that it will continue to be fulfilled at least a little longer. If we see part of a circle, we naturally expect that the form of the line will be maintained in the part hidden from us. If a body has moved uniformly over a certain space, we expect that it will continue to move uniformly. The ground of such inference is doubtless identical with that of all other inductive inferences. In continuous motion every infinitely small space passed over constitutes a separate constituent fact, and had we perfect powers of observation the smallest finite motion would include an infinity of information, which, by the principles of the inverse method of probabilities, would enable us to infer with actual certainty to the next infinitely small portion of the path. But when we attempt to infer from one finite portion of a path to another finite part, the inference will be only more or less probable, according to the comparative lengths of the parts and the accuracy of the observations; the longer our experience is, the more probable our inferences will be; the greater the length of time or space over which the inference extends, the less probable.

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[y] 'Philosophical Magazine,' 4th Series, vol. xlii. p. 451.

[z] Grove, 'Correlation of Physical Forces,' 3rd edit. p. 118.

[a] *Ibid.* pp. 166, 199, &c.

This principle of continuity presents itself in nature in a great variety of forms and cases. It is familiarly expressed in the dictum *Natura non agit per saltum,* in other words, no change in a natural phenomenon comes on with perfect suddenness or abruptness. There is always some notice—some forewarning of every phenomenon, and every change begins by insensible degrees, could we observe it with perfect accuracy. The cannon ball, indeed, is forced from the cannon in [illegible] reciable portion of time; the trigger is pull [illegible] he powder inflamed, the ball expelled, all simultaneously to our senses. But there is no doubt that time is occupied by every part of the process, and that the ball begins to move at first with indefinite slowness. Captain Noble is able to measure by his chronoscope the progress of the shot in a 300-pounder gun, and finds that the whole motion within the barrel takes something less than $\frac{1}{200}$ of a second. It is an invariable principle of nature that no finite force can produce motion, except in a finite space of time. The amount of momentum communicated to a body is proportional to the accelerating force multiplied by the time through which it acts uniformly. Thus a slight force produces a great velocity only by long continued action. In a powerful shock, like that of a railway collision, the stroke of a hammer on a hard anvil, or the discharge of a gun, the time is very short, and therefore the accelerating forces brought into play are exceedingly great, but never infinite. In the case of a large gun the powder in exploding is said to exert for a moment a force equivalent to at least 2,800,000 horses.

Our belief in some of the most fundamental laws of nature rests upon the principles of continuity. Galileo is held to be the first philosopher who consciously employed this principle in his arguments concerning the nature of motion, and it is certain that we can never by pure experience assure ourselves of the truth even of the first law of motion. *A material particle,* we are told, *when not acted on by extraneous forces will continue in the same state of rest or motion.* This may be true, but as we can find no body which is free from the action of extraneous causes, how are we to prove it? Only by observing that the less the amount of those forces the more nearly is the law found to be true. A ball rolled along rough ground is soon stopped; along a smooth pavement it continues longer in movement. A delicately suspended pendulum is almost free from friction against its supports, but it is gradually stopped by the resistance of the air; place it in the vacuous receiver of an air-pump and we find the motion immensely prolonged. A large planet like Jupiter experiences almost infinitely less friction, in comparison to its vast momentum, than we can produce experimentally, and we find through centuries that there is not the least evidence of the falsity of the law. Experience, then, informs us that we may approximate indefinitely to a uniform motion by sufficiently decreasing the disturbing forces. It is a pure act of inference which enables us to travel on beyond experience, and assert that, in the total absence of any extraneous force, motion would be absolutely uniform. The state of rest, again, is but a singular case in which motion is infinitely small or zero, to which we may attain, on the principle of continuity, by considering successively cases of slower and slower motion.

There are many interesting cases of physical phenomena, in which, by gradually passing from the apparent to the obscure, we can assure ourselves of the nature of phenomena which would otherwise be a matter of great doubt. Thus we can sufficiently prove, in the manner of Galileo, that a musical sound consists of rapid uniform pulses, by causing strokes to be made at intervals which we gradually diminish until the separate strokes coalesce into a uniform hum or note. With great advantage we approach, as Tyndall says, the sonorous through the grossly mechanical. In listening to a great organ we cannot fail to perceive that the longest pipes, or their partial tones, produce a tremor and fluttering of the building. At the other extremity of the scale, there is no fixed limit to the acuteness of sounds which we can hear; some individuals can hear sounds too shrill for other ears, and [illegible] ng in the nature of the atmosphere to prevent the existence of undulations incomparably more rapid than any of which we are conscious, we may infer, by the principle of continuity, that such undulations probably exist.

There are many habitual actions which we perform we know not how. So rapidly are many acts of mind accomplished that analysis seems impossible. We can only investigate them when in process of formation, observing that the best formed habit or instinct is slowly and continuously acquired, and it is in the early stages that we can perceive the rationale of the process.

Let it be observed that this principle of continuity must be held of much weight only in exact physical laws, those which doubtless repose ultimately upon the simple laws of motion. If we fearlessly apply the principle to all kinds of phenomena, we may often be right in our inference, but also often wrong. Thus, before the development of spectrum analysis, astronomers had observed that the more they increased the powers of their telescopes the more nebulæ they could resolve into distinct stars. This result had been so often found true that they almost irresistibly assumed that all the nebulæ would be ultimately resolved by telescopes of sufficient power; yet Mr. Huggins has in recent years proved by the spectroscope, that certain nebulæ are actually gaseous, and in a truly nebulous state. Even one such observation is a real exception sufficient to invalidate previous inferences as to the constitution of the universe.

The principle of continuity must have been continually employed in the inquiries of Galileo, Newton, and other experimental philosophers, but it appears to have been distinctly formulated for the first time by Leibnitz. He at least claims to have first spoken 'of the law of continuity' in a letter to Bayle, printed in the 'Nouvelles de *la République des Lettres*,' an extract from which is given in Erdmann's edition of Leibnitz' works, p. 104, under the title 'Sur un Principe Général utile à l'explication des Lois de la Nature[b].' It has indeed been asserted that the doctrine of the *latens processus* of Francis Bacon involves the principle of continuity[c], but I think that this doctrine, like that of the *natures* of substances is merely a vague statement of the principle of causation.

Failure of the Law of Continuity.

There are certain requisite cautions which must be given as to the application of the principle of continuity. In the first place, where this principle really holds true, it may seem to fail owing to our imperfect means of observation. Though a physical law may never admit of perfectly abrupt change, there is no limit to the approach which it may make to abruptness. When we warm a piece of very cold ice, the absorption of heat, the temperature, and the dilatation of the ice vary according to apparently simple laws until we come to the zero of the Centigrade scale. Everything is then changed; an enormous absorption of heat takes place without any rise of temperature, and the volume of the ice decreases as it changes into water. Unless most carefully investigated, this change appears perfectly abrupt; but accurate observation seems to show that there is a certain forewarning; the ice does not turn into water all at once, but through a small fraction of a degree the change is gradual. All the phenomena concerned, if measured very exactly, would be represented not by angular lines, but continuous curves, undergoing rapid flexures; and we may probably assert with safety that between whatever points of temperature we examined ice, there would be found some indication, doubtless almost infinitesimally small, of the apparently abrupt change which was to occur at a higher temperature. It might also be pointed out that all the most important and apparently simple physical laws, such as those of Boyle and Marriotte, Dalton and Gay-Lussac, &c., are only approximately true, and the divergences from observation are forewarnings of abrupt changes, which would otherwise break the law of continuity.

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[b] 'Life of Sir W. Hamilton,' p. 439.

[c] Powell's 'History of Natural Ph [illegible], 'Novum Organum,' bk. II. Aphorisms 5–7.

Secondly, it must be remembered that mathematical laws of any complexity will probably present singular cases or negative results, which may present the appearance of discontinuity, as when the law of refraction suddenly yields us with perfect abruptness the entirely different phenomenon of total internal reflection. In the undulatory theory there is no real change of law between the phenomenon of refraction and that of reflection.

Faraday in the earlier part of his career found so many substances possessing more or less magnetic power, that he ventured on a great generalization, and asserted that all bodies shared in the magnetic property of iron. His mistake, as he afterwards himself discovered, consisted in overlooking the fact that though magnetic in a certain sense, some substances might have negative magnetism, and be repelled instead of attracted by the magnet. Between magnetism and diamagnetism there must be a zero near or even at which some substances may be classed, but otherwise magnetic properties appear to be universally present in matter.

Thirdly, where we might expect to find a uniform mathematical law prevailing, the law may undergo abrupt change at singular points, and actual discontinuity may arise. We may sometimes be in danger of treating under one law phenomena which really belong to different laws. It is generally known, for instance, that a spherical shell of uniform matter attracts an external particle of matter with a force varying inversely as the square of the distance from the centre of the sphere. But this law only holds true so long as the particle is external to the shell. Within the shell the law is wholly different, and the aggregate gravity of the sphere becomes zero, because the force in every direction is neutralized by an exactly equal force. If an infinitely small particle be in the superficies of a sphere, the law is again different, and the attractive power of the shell is half what it would be on particles infinitely close to the surface of the shell. Thus in approaching the centre of a shell from a distance, the force of gravity evinces a double discontinuity in passing through the shell[d].

It may well admit of question, too, whether discontinuity is really unknown in nature. We perpetually do meet with events which are real breaks upon the previous law, though the discontinuity may then be a sign that some wholly independent cause has come into operation. If the ordinary course of the tides is interrupted by an enormous and irregular wave, we attribute it to an earthquake, or some gigantic natural disturbance. If a meteoric stone falls upon a person and kills him, it is clearly a discontinuity in his life, of which he could have had no anticipation. A sudden sound may pass through the air neither preceded nor followed by any continuous effect. Although, then, we may regard the Law of Continuity as a principle of nature holding rigorously true in many of the relations of natural forces, it seems to be a matter of difficulty to assign the limits within which the law is verified. Much caution, therefore, is desirable in its application.

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[d] Thomson and Tait, 'Treatise on N[illegible] l. i. pp. 346–351

Negative Arguments on the Principle of Continuity.

Upon the principle of continuity we may often found arguments of great force which prove an hypothesis to be impossible, because it would involve a continual repetition of a process *ad infinitum*, or else a purely arbitrary breach at some point. Bonnet's famous theory of reproduction represented every living creature as containing germs which were perfect representatives of the next generation, so that on the same principle they necessarily included germs of the next generation, and so on indefinitely. The theory was sufficiently refuted when once clearly stated, as in the following poem called the Universe[e], by Henry Baker:

> 'Each seed includes a plant: that plant, again,
> Has other seeds, which other plants contain:
> Those other plants have all their seeds, and those
> More plants again, successively inclose.
>
> 'Thus, ev'ry single berry that we find,
> Has, really, in itself whole forests of its kind,
> Empire and wealth one acorn may dispense,
> By fleets to sail a thousand ages hence.'

The general principle of inference, that what we know of one case must be true of similar cases, if they really are identical in the essential conditions, prevents our asserting anything which we cannot apply time after time under the same circumstances. On this principle Stevinus beautifully demonstrated that weights resting on two inclined planes and balancing each other must be proportional to the lengths of the planes between their apex and a horizontal plane. He imagined an uniform endless chain to be hung over the planes, and to hang below in a symmetrical festoon. If the chain were ever to move by gravity, there would be the same reason for its moving on for ever, and thus producing a perpetual motion. As this is absurd, the portions of the chain lying on the planes, and equal in length to the planes, must balance each other. On similar grounds we may disprove the existence of any *self-moving machine*, for if it could once alter its own state of motion or rest, in however small a degree, there is no reason why it should not do the like time after time *ad infinitum.* Even Newton's proof of his third law of motion, in the case of gravity, is of this character. For he remarks that if two gravitating bodies do not exert exactly equal forces in opposite directions, the one exerting the strongest pull will carry both itself and the other away, and will move with constantly increasing velocity *ad infinitum.* But though the argument might seem sufficiently convincing, Newton in his characteristic way made an experiment with a loadstone and iron floated upon the surface of water[f]. In recent years the very foundation of the principle of conservation of energy has been placed on the assumption that it is impossible by any combination whatever of natural bodies to produce force continually from nothing[g]. The principle admits of frequent application in various subtle forms.

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[e] 'Philosophical Transactions' (1740), vol. xli. p. 454.

Lucretius attempted to prove, by a most ingenious argument of this kind, that matter must be indestructible. For if a definite quantity, however small, were to fall out of existence in any finite time, an equal quantity might be supposed to lapse in every equal interval of time, so that in the infinity of past time the universe must have ceased to exist[h]. But the argument, however ingenious, besetting error of most first attempts at scientific research. The faculty to generalize accurately and philosophically requires large caution and long training; and is not fully attained, especially in reference to more general views, even by some who may properly claim the title of very accurate scientific observers in a more limited field. It is an intellectual habit which acquires immense and accumulating force from the contemplation of wider analogies.'

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[f] 'Principia,' bk. I. Law iii. Corollary 6.

[g] Helmholtz, Taylor's 'Scientific Memoirs' (1853), vol. vi. p. 118.

[h] 'Lucretius,' bk. I. lines 232–264.

Hasty and superficial generalizations have always been the bane of science, and there would be no difficulty in finding endless illustrations. Between things which are the same in number there is a certain resemblance, namely in number, but in the infancy of science men could not be persuaded that there was not a deeper resemblance implied in that of number. Pythagoras was not the inventor of a mystical science of number. In the ancient Oriental religions the seven metals were connected with the seven planets, and in the seven days of the week we still have, and probably always shall have, a relic of the septiform system ascribed by Dio Cassius to the ancient Egyptians. The disciples of Pythagoras carried the doctrine of the number seven into great detail. Seven days are mentioned in Genesis; infants acquire their teeth at the end of seven months; they change them at the end of seven years; seven feet was the limit of man's height; every seventh year was a climacteric or critical year, at which a change of disposition took place. Then again there were the seven sages of Greece, the seven wonders of the world, the seven rites of the Grecian games, the seven gates of Thebes, and the seven generals destined to conquer that city.

In natural science there were not only the seven planets, and the seven metals, but also the seven primitive colours, and the se[illegible]es of music. So deep a hold did this doctrine take that we still have its results in many customs, not only in the seven days of the week, but the seven years' apprenticeship, puberty at fourteen years, the second climacteric, and legal majority at twenty-one years, the third climacteric. The system was reproduced in the seven sacraments of the Roman Catholic Church, and the seven year periods of Comte's grotesque system of domestic worship. Even in scientific matters the loftiest intellects have occasionally yielded, as when Newton was misled by the analogy between the seven tones of music and the seven colours of his spectrum. Other numerical analogies, though rejected by Galileo, held Kepler in thraldom; no small part of Kepler's labours during seventeen years was spent upon numerical and geometrical analogies of the most baseless character; and he gravely held that there could not be more than six planets, because there were not more than five regular solids. Even the acute genius of Huyghens did not prevent him from inferring that but one satellite could belong to Saturn, because, with those of Jupiter and the Earth, it completed the perfect number of six. A whole series of other superstitions and fallacies attach to the numbers six and nine[l].

It is by false generalization, again, that the laws of nature have been supposed always to possess that simplicity and perfection which we attribute to particular forms and relations. The heavenly bodies, it was held, must move in circles, for the circle was the perfect figure. Even Newton seemed to adopt the questionable axiom that nature must always proceed in the simplest way; in stating his first rule of philosophizing, he adds[m]: 'To this purpose the philosophers say, that nature does nothing in vain, when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes.' Keill, again, lays down[n] as an axiom that 'The causes of natural things are such, as are the most simple, and are sufficient to explain the phenomena: for nature always proceeds in the simplest and most expeditious method; because by this manner of operating the Divine Wisdom displays itself the more.' If this axiom had any clear grounds of truth, it would not apply to proximate laws; for even when the ultimate law may appear simple the results may be infinitely diverse, as in the various elliptic, hyperbolic, parabolic, or circular orbits of the heavenly bodies. Simplicity is naturally agreeable to a mind of very finite powers, but to an Infinite Mind everything is simple.

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[l] Baring-Gould, 'On the Fatalities of Number,' in 'Curious Myths of the Middle Ages' (1866), p. 209.

[m] 'Principia,' bk. III. *ad initium.*

Every great advance in science consists in a great generalization, pointing out deep and subtle resemblances. The Copernican system was a generalization, in that it classed the earth among the planets; it was, as Bishop Wilkins expressed it, 'the discovery of a new planet,' but it was opposed by a more shallow generalization. Those who argued from the condition of things upon the earth's surface, thought that every object must be attached to and rest upon something else. Shall the earth, they said, alone be free? Accustomed to certain special results of gravity they could not conceive its action under widely different circumstances[o]. No hasty thinker could seize the deep analogy pointed out by Horrocks between a pendulum and a planet, true in substance though mistaken in some details. All the advances of modern science rise from the conception of Galileo, that in the heavenly bodies, however apparently different their condition, we shall ultimately recognise the same fundamental principles of mechanical science which are true on earth.

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[n] Keill, 'Introduction to Natural Philosophy,' p. 89.

[o] Jeremiæ Horroccii (1673), pp. 26, 27.

Generalization is the great prerogative of the intellect, but it is a power only to be exercised safely with much caution and after long training. Every mind must generalize, but there are the widest differences in the depth of the resemblances discovered and the care with which the discovery is verified. There seems to be an innate power of insight which a few men have possessed pre-eminently, and which enabled them, with no exemption indeed from labour or temporary error, to discover the one in the many. Minds of excessive acuteness may exist, which have yet only the powers of minute discrimination, and of storing up, in the treasure-house of memory, vast accumulations of words and incidents. But the power of discovery belongs to a more restricted class of minds. Laplace said that, of all inventors who had contributed the most to the advancement of human knowledge, Newton and Lagrange appeared to possess in the highest degree the happy tact of distinguishing general principles among a multitude of objects enveloping them, and this tact he conceived to be the true characteristic of scientific genius[o].

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[o] Young's 'Works,' vol. ii. p. 564.

CHAPTER XXVIII.

ANALOGY.

As we have seen in the previous chapter, generalization passes insensibly into reasoning by analogy, and the difference is but one of degree. We are said to generalize when we view many objects as agreeing in a few properties, so that the resemblance is extensive rather than deep. When we have two or only a few objects of thought, but are able to discover many points of resemblance, we argue by analogy that the correspondence will be even deeper than appears. It may not be true that the words are always used in these distinct senses, and there is no doubt great vagueness in the employment of these and many other logical terms; but, if there is any clear discrimination to be drawn between generalization and analogy, it is indicated above.

It has been often said, indeed, that analogy denotes not a resemblance between things, but between the relations of things. A pilot is a very different man from a Prime Minister, but he bears the same relation to a ship that the minister does to the state, so that we may analogically [illegible] the [illegible] Minister as the pilot of the state. A [illegible] a horse, nevertheless four men [illegible] relation as four horses bear [illegible] analogy.

Fo[illegible] horses : Three horses, or Fo[illegible] men : Three horses. There [illegible] tones of the Monochord, the Sages of Thebes, and the Gates of Thebes, but it does not extend beyond the fact that they were all seven in number. Between the most discrete notions, as, for instance, those of time and space, analogy may exist, arising from the fact that the mathematical conditions of the lapse of time and of motion along a line are similar. There is no identity of nature between a word and the thing it signifies; the substance *iron* is a heavy solid, the word *iron* is either a momentary disturbance of the air, or a film of black pigment on white paper; but there is analogy between words and their significates. The substance iron is to the substance iron-carbonate, as the name iron is to the name iron-carbonate, when these names are used according to their correct scientific definitions. The whole structure of language and the whole utility of signs, marks, symbols, pictures, and representations of various kinds, rest upon analogy. I may, perhaps, hope to enter fully upon this important subject at some future time, and to attempt to show how the invention of signs enables us to express, guide, and register our thoughts. It will be sufficient to observe here that the use of words constantly involves analogies of a subtle kind; we should often be at a loss how to describe a notion, were we not at liberty to employ in a metaphorical sense the name of anything sufficiently resembling it. There would be no expression for the sweetness of a melody, or the brilliance of an harangue, unless it were furnished by the taste of honey and the brightness of a torch.

A very cursory examination of the cases in which we popularly use the word analogy, shows that it includes all degrees of resemblance or similarity. The analogy may consist only in similarity of number or ratio; or in like relations of time or space. It may also consist in more simple resemblance between physical properties. We should not be using the word inconsistently with custom, if we said that there was an analogy between iron, nickel, and cobalt, manifested in the strength of their magnetic powers. There is a still more perfect analogy between iodine and chlorine; not that every property of iodine is identical with the corresponding property of chlorine; for then they would be one and the same kind of substance, and not two substances; but every property of iodine resembles in all but degree some property of chlorine. For almost every substance in which iodine forms a component, a corresponding substance may be discovered containing chlorine, so that we may confidently infer from the compounds of the one to the compounds of the other substance. Potassium iodide crystallizes in cubes; therefore it is to be expected that potassium chloride will also crystallize in cubes. The science of chemistry, as now developed, rests almost entirely upon a careful and most extensive comparison of the properties of substances, bringing to light deep-lying analogies. When any apparently exceptional or new substance is encountered, the chemist is guided in his treatment of it entirely by the analogies which it seems to present with previously known substances.

In this chapter I cannot hope to illustrate the all-pervading influence of analogy in human thought and science. All science, it has been said, at the outset, arises from the discovery of identity, and analogy is but one name by which we denote the deeper-lying cases of resemblance. I shall only try to point out at present how analogy between apparently diverse classes of phenomena often serves as an all-important guide in discovery. We thus commonly gain the first insight into the nature of an apparently unique object, and we thus, in the progress of a science, often discover that we are treating over again, in a new form, phenomena which were well known to us in another form

Analogy as a Guide in Discovery.

There can be no doubt that discovery is most frequently accomplished by following up hints received from analogy, as Jeremy Bentham remarked[a]. Whenever a phenomenon is perceived, the first impulse of the mind is to connect it with the most nearly similar phenomenon. If we could ever meet a thing wholly *sui generis*, presenting no analogy to anything else, we should be incapable of investigating its nature, except by purely haphazard trial. The probability of success by such a process is so slight, that it is preferable to follow up the slightest clue. As I have pointed out already (vol. ii. p. 24), the possible modifications of condition in experiments are usually infinite in number, and infinitely numerous also are the hypotheses upon which we may proceed. Now it is self-evident that, however slightly superior the probability of success by one course of procedure may be over another, the most probable one should always be adopted first.

The chemist having discovered what he believes to be a new element, will have an infinite variety of modes of treating and investigating it. If in any one of its qualities the substance displays a resemblance to an alkaline metal, for instance, he will naturally proceed to try whether it possesses other properties common to the alkaline metals. Even the apparently simplest phenomenon presents so many points for notice that we have a choice at each moment from among many hypotheses.

It would be difficult to find a more instructive instance of the way in which the mind is guided by analogy than in the description by Sir John Herschel of the course of thought by which he was led to anticipate in theory one of Faraday's greatest experimental discoveries. Sir John

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[a] 'Essay on Logic,' 'Works,' vol. viii. p. 276.

Herschel noticed that in three physical phenomena, a screw-like form, technically called helicoidal dissymmetry, was presented, namely in electrical helices, plagihedral quartz crystals, and the rotation of the plane of polarization of light. As he himself has said[b], 'I reasoned thus: Here are three phenomena agreeing in a *very strange peculiarity.* Probably, this peculiarity is a connecting link, physically speaking, among them. Now, in the case of the crystals and the light, this probability has been turned into certainty by my own experiments. Therefore, induction led me to conclude that a similar connexion exists, and must turn up, somehow or other, between the electric current and polarized light, and that the plane of polarization would be deflected by magneto-electricity.' By this course of analogical thought Sir John Herschel had actually been led to anticipate Faraday's great discovery of the influence of magnetic strain upon polarized light. He had tried as long ago as 1822–25 to discover the influence of electricity on light, by sending a ray of polarized light through a helix, or near a long wire conveying an electric current. Such a course of inquiry, followed up with the persistency of Faraday, and with his experimental resources, would doubtless have effected the strange discovery. Herschel also suggests that the plagihedral form of quartz crystals must be due to a screw-like strain during the progress of crystallization; but the notion, although probable, remains unverified by experiment to the present day.

If ever men approach the investigation of the mechanism of thought, they must be guided by analogy. Already many philosophers have drawn analogies between nerve influence and the transmission of vibrations. Dr. Briggs, Newton in his 24th Query, and Hartley, have vaguely speculated concerning such vibrations, and some countenance is now given to the notion by the somewhat similar rate of propagation of nerve pulses and sound-waves in soft bodies. But the phenomena of memory are far more difficult to reduce to any material mechanism, and I know of no material analogy but the interesting one suggested by Hooke[c], who likens memory to 'those bells or vases which Vitruvius mentions to be placed in the ancient theatre, which did receive and return the sound more vigorous and strong; or like the unison-toned strings, bells, or glasses, which receive impressions from sounds without, and retain the impressions for some time, answering the tone by the same tone of their own.'

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[b] 'Life of Faraday,' by Bence Jones, vol. ii. p. 206.

Analogy in the Mathematical Sciences.

Whoever wishes to acquire a deep acquaintance with the constitution of Nature must observe that there are deep analogies which connect whole branches of science in a parallel manner, and enable us to infer of one class of phenomena what we know of the other. It has thus happened on several occasions that the discovery of an unsuspected analogy between two hitherto distinct branches of knowledge has been the starting-point for a rapid course of discovery. The truths readily observed in the one may be of a different character from those which present themselves in the other. The analogy, when once pointed out, leads us easily to discover regions of one science yet undeveloped, but to which the key is furnished by the corresponding truths in the other science. An interchange of aid most wonderful in its results may thus take place, and at the same time the mind rises to a higher generalization, and a more comprehensive view of mind and nature.

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[c] 'Posthumous Works,' p. 141.

No two sciences might seem at first sight more entirely discrete and divergent in their subject matter than geometry and arithmetic, or algebra. The first deals with circles, squares, parallelograms, and various other forms in space; the latter with mere symbols of number, the symbols having form indeed, but bearing a meaning independent of shape or size. Prior to the time of Descartes, too, the sciences actually were developed in a slow and painful manner in almost entire independence of each other. The Greek philosophers indeed could not avoid noticing occasional analogies, as when Plato in the Thæetetus describes a square number as *equally equal*, and a number produced by multiplying two unequal factors as *oblong*. Euclid, in the 7th and 8th books of his Elements, continually uses expressions displaying a consciousness of the same analogies, as when he calls a number of two factors a *plane number*, ἐπίπεδος ἀριθμός, and distinguishes a square number of which the two factors are equal as an equal-sided or plane number, ἰσόπλευρος καὶ ἐπίπεδος ἀριθμός. He also calls the root of a cubic number its side, πλευρά. In the Diophantine algebra many problems of a geometrical character were solved by algebraic or numerical processes; but there was no general system, so that the solutions were of an isolated character. In general the ancients were far more advanced in geometric than symbolic methods; thus Euclid in his 4th book gives us the means of dividing a circle by purely geometric or mechanical means into 2, 3, 4, 5, 6, 8, 10, 12, 15, 20, 24, 30 parts, but he was totally unacquainted with the theory of the roots of unity exactly corresponding to this division of the circle.

During the middle ages, on the other hand, algebra advan [illegible] and modes of solving equations w [illegible] who had no notion [illegible] ving important geometric problems. It is true that Regiomontanus, Tartaglia, Bombelli, and possibly other early algebraists, solved isolated geometrical problems by the aid of algebra, but particular numbers were always used, and no consciousness of a general method was displayed. Vieta in some degree anticipated the final discovery, and occasionally represented the roots of an equation geometrically, but it was reserved for Descartes to show, in the most general manner, that every equation may be represented by some curve or figure in space, and that every bend, point, cusp, or other peculiarity in the curve indicates some peculiarity in the values of the algebraic symbols. It is impossible to describe in any adequate manner the importance of this discovery. The advantage was twofold: algebra aided geometry, and geometry gave reciprocal aid to algebra. Curves such as the long described sections of the cone were found to correspond to quadratic equations of no great difficulty; and it was impossible to manipulate the symbolic equations without discovering properties of those all important curves. The way was thus opened for the algebraic treatment of motions and forces, without which Newton's 'Principia' could never have been worked out. Newton indeed was possessed by a strange and, to some extent, unfortunate infatuation in favour of the ancient geometrical methods; but it is well known that he employed symbolic methods to discover his profound truths, and he every now and then, by some accidental use of algebraic expressions, confessed its greater powers and generality.

Geometry, on the other hand, gave the greatest assistance to algebra, by affording concrete representations of relations which would otherwise be too abstract for easy comprehension. A curve of no great complexity may give the whole history of the variations of value of a troublesome mathematical expression. As soon as we know, too, that every regular geometrical curve represents some algebraic equation, we are presented by simple observation of many mechanical movements with abundant suggestions towards the discovery of mathematical problems. Every particle of a carriage-wheel when moving on a level road is constantly describing a cycloidal curve, the curious properties of which exercised the ingenuity of all the most skilful mathematicians of the seventeenth century, and led to important advancements in algebraic power. It may well be held even that the discovery of the Differential Calculus is mainly due to geometrical analogy, because mathematicians, in attempting to treat algebraically the tangent of a continuously varying curve, were obliged to entertain the notion of infinitely small quantities[d]. There can be no doubt that Newton's fluxional, or in fact geometrical mode of stating the differential calculus, however much it subsequently retarded its progress in England, facilitated its apprehension at first, and I should think it almost certain that Newton discovered the calculus geometrically.

We may accordingly look upon this discovery of analogy, this happy alliance, as Bossut calls it[e], between geometry and algebra, as the chief source of discoveries which have been made for three centuries past in mathematical methods. This is certainly the opinion of no less an authority than Lagrange, who has said, 'So long as algebra and geometry have been separate, their progress was slow, and their employment limited; but since these two sciences have been united, they have lent each other mutual strength, and have marched together with a rapid step towards perfection.'

The advancement of mechanical science has also been greatly aided by analogy. An abstract and intangible existence like force demands much power of conception, but it has a perfect concrete representative in a line, the end of which may denote the point of application, and the direction the line of action of the force, while the length can be made arbitrarily to denote the amount of the force. Nor does the analogy end here; for the moment of the force about any point, or its product into the perpendicular distance of its line of action from the point, is found to be correctly represented by an area, namely twice the area of the triangle contained between the point and the ends of the line representing the force. Of late years a great generalization has been effected; the Double Algebra of De Morgan is true not only of space relations, but of forces, so that the triangle of forces is reduced to a case of pure geometrical addition. Nay, the triangle of lines, the triangle of velocities, the triangle of forces, the triangle of couples, and perhaps other cognate theorems, are reduced by analogy to one simple theorem, which amounts merely to this, that there are two ways of getting from one angular point of a triangle to another, which ways, though different in length, are identical in their final results[f]. In the wonderful system of quaternions of the late Sir W. R. Hamilton, these analogies are embodied and carried out in the most general manner, so that whatever problem involves the threefold dimensions of space, or relations analogous to those of space, is treated by a symbolic method of the most comprehensive simplicity. Since nearly all physical problems do involve space relations, or those analogous to them, it is difficult to imagine any limits to the work which may be ultimately achieved by this calculus.

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[d] Lacroix, 'Traité Elémentaire de Calc [illegible] et de Calcul Intégral,' 5^{me} édit. p. 699.

[e] 'Histoire des Mathématiques,' [illegible]

It ought to be added that to the discovery of analogy between the forms of mathematical and logical expressions, we undoubtedly owe the greatest recent advance in logical science. Boole based his extension of logical processes entirely upon the notion that logic was an algebra of two quantities, 0 and 1. His profound genius for the investigation of symbolic methods led him to perceive by analogy that there must exist a general system of logical deduction, of which the old logicians had seized only a few stray fragments. Much mistaken as he was in placing algebra as a higher science than logic, no one can deny that the development of the more complex and dependent science had advanced far beyond that of the simpler science, and that Boole, in drawing attention to the connexion, made one of the most important discoveries in the history of science. As Descartes had wedded algebra and geometry, so did Boole substantially accomplish the marriage of logic and algebra.

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[f] See Goodwin, 'Cambridge Philosophical Transactions' (1845), vol. viii. p. 269. O'Brien, 'On Symbolical Statics,' Philosophical Magazine, 4th Series, vol. i. pp. 491 &c.

Analogy in the Theory of Undulations.

There is no class of phenomena which more thoroughly illustrates alike the power and weakness of analogy than the waves which agitate every kind of medium. All waves, whatsoever be the matter through which they pass, obey certain common principles of rhythmical or harmonic motion, and the subject therefore presents a vast field for mathematical generalization. At the same time each kind of medium may allow of waves peculiar in their conditions, so that it is a beautiful exercise in analogical reasoning to observe how, in making inferences from one kind of medium to another, we must make allowance for difference of circumstances. The waves of the oce [illegible] visible, and there are the yet greate [illegible] nd around the globe. From s [illegible] d movement we pass by analogy to waves of sound, varying in length from about 32 feet to a small fraction of an inch. We have but to imagine, if we can, the fortieth octave of the middle C of a piano, and we reach the undulations of yellow light, the ultra-violet being about the forty-first octave. Thus we pass gradually from the palpable and evident to that which is obscure, if not incomprehensible. Yet the very same phenomena of reflection, interference, and refraction, which we find in the one case, may be expected to occur *mutatis mutandis* in the other cases.

From the great to the small, from the evident to the obscure, is not only the natural order in which inference proceeds, but it is the historical order of discovery. The physical science of the Greek philosophers must have remained incomplete, and their theories groundless, because they do not seem ever to have understood the nature and importance of undulations. All their systems were therefore based upon the entirely different notion of continuous movement of translation from place to place. Modern Science tends more and more to the opposite conclusion that all motion is alternating or rhythmical, energy flowing onwards but matter remaining comparatively fixed in position. Diogenes Laertius indeed correctly compared the propagation of sound with the spreading of waves on the surface of water when disturbed by a stone, and Vitruvius displayed a more complete comprehension of the same analogy. It remained for Newton to create the theory of undulatory motion in showing by mathematical deductive reasoning that the particles of an elastic fluid, by vibrating backwards and forwards, might carry forward a pulse or wave moving onwards from the source of disturbance, while the disturbed particles return to their place of rest. He was even able to make a first approximation by theoretical calculation to the velocity of sound-waves in the atmosphere. His theory of sound formed a hardly less important epoch in science than his far more celebrated theory of gravitation. It opened the way to all the subsequent applications of mechanical principles to the insensible motion of molecules. He seemed to have been frequently, too, upon the brink of another application of the same principles which would have advanced science by at least a century of progress, and made him the undisputed founder of all the theories of matter. He expressed opinions at various times that light might be due to undulatory movements of a medium occupying space, and in one intensely interesting sentence remarks[g] that colours are probably vibrations of different lengths, 'much after the manner that, in the sense of hearing, nature makes use of aërial vibrations of several bignesses to generate sounds of divers tones, for the analogy of nature is to be observed'. He correctly foresaw that red and yellow light would consist of the longer undulations, and blue and violet of the shorter, while white light would be composed of an indiscriminate mixture of waves of various lengths. Newton almost overcame one of the strongest apparent difficulties of the undulatory theory of light, namely, the propagation of light in straight lines. For he observed that though waves of sound bend round an obstacle to some extent, they do not do so in the same degree as water-waves[h]. He had but to extend the analogy proportionally to light-waves, and not only would the difficulty have vanished, but the true theory of diffraction would have been open to him. Unfortunately he had a preconceived theory that rays of light are bent from and not towards the shadow of a body, a theory which for once he did not sufficiently compare with observation to detect its falsit [illegible] not aware, too, that

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[g] Birch, 'History of the [illegible] iii. p. 262, quoted by Young, 'Works,' vol. i. p. 1 [illegible]

[h] 'Opticks,' Query 28, 3 [illegible]

Newton has, in any of his works, displayed an understanding of the phenomena of interference inseparable from the notion of waves.

While the general principles of undulatory or harmonic motion will be the same in whatever medium the motion takes place, the circumstances must often be excessively different. Between light travelling 186,000 miles per second and sound travelling in air only about 1100 feet in the same unit of time, or almost 900,000 times as slowly, we cannot expect a close outward resemblance. There are great differences, too, in the character of the vibrations. Gases scarcely admit of transverse vibration, so that sound travelling in air is a longitudinal wave, the particles of air moving backwards and forwards in the same line in which the wave moves onwards. Light, on the other hand, appears to consist entirely in the movement of points of force transversely to the direction of propagation of the ray. The light-wave is partially analogous to the bending of a rod or of a stretched cord agitated at one end. Now this bending motion may take place in any one of an infinite number of planes, and waves of which the planes are perpendicular to each other cannot interfere any more than two perpendicular forces can interfere. Now the whole of the complicated phenomena of polarized light arise out of this transverse character of the luminous wave, and we must not expect to meet any analogous phenomena in atmospheric sound-waves. It is conceivable that in solids we might produce transverse sound undulations, in which many of the phenomena of polarization might be reproduced. But it would appear that even between transverse sound and light-wave
analogy holds true rather of the principles of ha
motion than the circumstances of the vibrating
from experiment and theory it is inferred that
of polarization in plane polarized light is

instead of being coincident with the direction of vibration, as it would be in the case of transverse sound undulations. Thus the laws of elastic forces appear to be essentially different in application to the luminiferous ether and to ordinary solid bodies[i].

Between light and heat, forms of energy, which at first sight appear so different, a perfect analogy has gradually been established. Not only do rays of light and heat obey exactly the same laws of reflection and refraction, but they are subject to exactly the same laws of absorption and polarization. Wherever a light-ray is deficient in the solar spectrum, a heat-ray is also missing. It is now considered that light is but the influence of heat-rays of certain wave-lengths upon the eye, so that we may in fact cease to distinguish radiant heat and rays of light. Heat in the radiant condition is, of course, to be distinguished from the molecular vibration also called heat, and from the potential energy which it produces when absorbed by substances, and rendered latent.

Use of Analogy in Astronomy.

We shall be much assisted in gaining a true appreciation of the value of analogy in its feebler degrees, by considering how much it has contributed to the progress of astronomical science. Our point of observation is so fixed with regard to the universe, and our means of examining distant bodies is so restricted, that we are obliged in many cases to be guided by limited and apparently feeble resemblances. In many cases the result has been confi[illegible] subsequ[illegible] [illegible]rect evidence of the most [illegible] was divided in opinion

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[illegible] (1856) [illegible] xlvi. p. 282.

between the Copernican, and Ptolemaic systems, it was analogy which furnished the most satisfactory arguments. Galileo discovered, by the use of his new telescope, the four small satellites which circulate round Jupiter, and make a miniature planetary world. These four Medicean Stars, as they were called, were plainly seen to revolve round Jupiter in various periods, but approximately in one plane, and astronomers irresistibly inferred that what might happen on the smaller scale might also be found true of the greater planetary system. This discovery gave the holding turn, as Sir John Herschel has expressed it, to the opinions of mankind. Even Francis Bacon, who had, in a manner little to the credit of his scientific sagacity, previously opposed the Copernican views, now became partially convinced, saying 'We affirm the solisequium of Venus and Mercury; since it has been found by Galileo that Jupiter also has attendants.' Nor did Huyghens think it superfluous to adopt the analogy as a valid argument[k]. Even in an advanced stage of the science of physical astronomy, the Jovian system has not lost its analogical interest; for the mutual perturbations of the four satellites pass through all their phases within a few centuries, and thus enable us to verify in a miniature case the principles of stability, which Laplace has established for the great planetary system. Oscillations or disturbances which in the motions of the planets appear to be secular, because their periods extend over millions of years, can be watched, in the case of Jupiter's satellites, through complete revolutions within the historical periods of astronomy[l].

In obtaining a knowledge of the stellar universe we must depend much upon somewhat precarious analogies. We must start with the opinion, entertained by Bruno, as long ago as 1591, that the stars may be suns attended perhaps by planets like our earth. This is the most probable first assumption, supported in some degree by very recent spectrum observations, which show the similarity of light derived from many stars with that of the sun. But at the same time we learn by the prism that there are nebulæ and stars in conditions widely different from anything known in our system. In the course of time the analogy may perhaps be restored to comparative completeness by the discovery of many suns in various stages of nebulous condensation. The history of the evolution of our own world may, as it were, be traced back in bodies less developed, or traced forwards in systems more advanced towards the dissipation of energy, and the extinction of life. As in a great workshop, we may perhaps see the material work of Creation as it has variously progressed through thousands of millions of years.

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[k] 'Cosmotheoros' (1699), p. 16.

[l] Laplace, 'System of the World,' vol. ii.

By the careful delineation and classification of the nebulæ and stellar systems, we may hope in time to find some parallel even to that apparently space-filling system of the Milky Way. Michell pointed out that the Pleiades form a remarkable group of worlds, and he thought that it might present an analogy to the sun and its immediate neighbours. The observations of the Herschels and other more recent astronomers, show that we really belong to a vast stratum of worlds of a peculiar split form, including countless myriads of stars of various sizes. The belief in analogy is irresistible, and astronomers have already looked into the depths of space, hoping to find distant [illegible] specks which might resemble the supposed [illegible] Milky [illegible] nd extend our knowledge [illegible] order [illegible] Such expectations are pr[illegible] ture, [illegible] founded ; nevertheless in [illegible] the [illegible] may find much in[illegible] d f[illegible] l in many bodies by Lord Rosse's telescope possesses some analogy to what would happen in a system revolving in a dense retarding medium. Let us once ascertain by the spectroscope that there is a dense envelope of gas, and the forms of those bodies are at once brought into harmony with the laws of matter on this globe. Viewing such worlds as we do from a fixed distant point, they appear variously distorted according to the laws of perspective; but when we find in many objects forms which might have proceeded from the same object variously inclined to the line of vision, analogy will aid us in determining the real form. Thus when we see an apparent nebulous ring, we may be unable to decide whether it is really a ring of matter or a spherical shell, of which the obliquely seen edges are alone apparent. But if elsewhere we discover, as did Lord Rosse, another nebula presenting the distinct appearance of a ring seen edgeways, we may infer with some probability from one case to the other. By similar processes of comparison and analogical reasoning, we may in time assign with much confidence the absolute forms of many classes of celestial objects[m].

In speculations concerning the physical condition of other planets and heavenly bodies, we must often depend upon analogies of a very weak character. We may be said to know that the moon has mountains and valleys, plains and ridges, volcanoes, and streams of lava, and, in spite of the absence of air and water, the rocky surface of the moon presents so many familiar appearances that we do not hesitate to compare them with the features of our own globe. We infer with high probability that Mars has polar snow and an atmosphere absorbing blue rays like our own; Jupiter undoubtedly possesses a cloudy atmosphere, possibly not unlike a magnified copy of t surrounding the earth, but our tendency to adopt alogies receives a salutary correction in the recently discovered fact that the atmosphere of Uranus contains hydrogen. Philosophers of the highest grade have not stopped at these comparatively safe inferences, but have speculated on the existence of living creatures in other planets. Huyghens remarked that as we infer by analogy from the dissected body of a dog to that of a pig and ox or other animal of the same general form, and as we expect to find the same viscera, the heart, stomach, lungs, intestines, &c., in corresponding positions, so when we notice the similarity of the planets in many respects, we must expect to find them alike in other points[n]. He even enters into an inquiry whether the inhabitants of other planets would possess reason and knowledge of the same sort as ours, concluding in the affirmative. Although the power of intellect might be different, he considers that they would have the same geometry if they had any at all, and that what is true with us would be true with them[o]. As regards the sun, he wisely observes that every conjecture fails. Laplace entertained a strong belief in the existence of inhabitants on other planets. The benign influence of the sun gives birth to animals and plants upon the surface of the earth, and analogy induces us to believe that his rays would tend to have a similar effect elsewhere. It is not probable that matter which is here so fruitful of life, would be sterile u[illegible] globe as Jupiter, which, like the earth, has [illegible] and years, and changes which indicat[illegible] indeed is formed for the temperatu[illegible] which he lives, and, so far as appear[illegible] the other planets. But th[illegible] organizations relative to th[illegible] bodies of the universe. Th[illegible] not form any idea of such various creatures, but their existence is not unlikely[p].

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[m] Grant's 'History of Physical Astronomy,' pp. 570–571.

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[n] 'Cosmotheoro[illegible]

[o] Ibid p. 36.

We now know that many metals and other elements never found in organic structures are yet capable of forming compounds, with substances of vegetable or animal origin. It is therefore just possible that at different temperatures creatures formed of different but analogous compounds might exist, but it would seem indispensable that carbon should still form the basis of organic structures; for we have no analogies to lead us to suppose that in the absence of that complex element, life can exist. Could we find globes surrounded by atmospheres resembling our own in temperature and composition, we should be almost forced to believe them inhabited, but the probability of any analogical argument decreases rapidly as the condition of a globe diverges from that of our own. The Cardinal Nicholas de Cusa held long ago that the moon was inhabited, but the absence of any appreciable atmosphere renders the existence of inhabitants highly improbable. Speculations resting upon weak analogies hardly belong to the scope of true science, and can only be tolerated as an antidote to the far worse dogmatism which would assert that the thousand million of persons [illegible] th, or rather a small fraction of them, are the [illegible] of care of the Power which designed this limit[illegible]

Failures of [illegible]

So constant is the aid which we [illegible] from [illegible] analogy in all attempts at discov[illegible] [illegible] it is most important to observe in [illegible] it [illegible] us into difficulties. That which we [illegible] exist may—

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[p] 'System of the World,' vol. ii. p. 326. 'E[illegible]

(1) Be found to exist;

(2) May seem not to exist, but nevertheless may really exist;

(3) May actually be non-existent.

In the second case the failure is only apparent, and arises from our obtuseness of perception, the smallness of the phenomenon to be noticed, or the disguised character in which it appears. I have already pointed out that the analogy of sound and light seems to fail because light does not bend round a corner, the fact being that it does so bend in the phenomena of diffraction, which present the effect, however, in such an unexpected and minute form, that even Newton was misled, and turned from the correct hypothesis of undulations which he had partially entertained.

In the third class of cases analogy fails us altogether, and we expect that to exist which really does not exist. Thus we fail to discover the phenomena of polarization in sound travelling through the atmosphere, since air is not capable of any appreciable transverse undulations. These failures of analogy are of peculiar interest, because they make the mind aware of its superior powers. There have been many philosophers who said that we can conceive nothing in the intellect which we have not previously received throug [illegible]. This is true in the se that we cannot [illegible] to the mind in the con form of a shap [illegible] we can speak of them reason concern [illegible] short, we often know in every [illegible] nner. Accurate in gation [illegible] substances retard motion [illegible] by substracting en by imp [illegible] ly we can fram notion of [illegible] there is no resis whatever, [illegible] for we have on proce [illegible] a medium s accelerate the motion of bodies passing through it, somewhat in the mode which Aristotelians attributed falsely to the air. Thus we can frame the notion of *negative density*, and Newton could reason exactly concerning it, although no such thing exists[q].

In every direction of thought we may meet ultimately with similar failures of analogy. A moving point generates a line, a moving line generates a surface, a moving surface generates a solid, but what does a moving solid generate? When we compare a polyhedron, or many-sided solid, with a polygon, or plane figure of many sides, the volume of the first is analogous to the area of the second; the face of the solid answers to the side of the polygon; the edge of the solid to the point of the figure; but the corner, or junction of edges in the polyhedron, is left wholly unrepresented in the plane of the polygon. Even if we attempted to draw the analogies in some other manner, we should still find a geometrical notion embodied in the solid which has no representative in the plain figure[r].

Faraday was able to frame some notion of matter in a fourth condition, which should be to gas what gas is to liquid[s]. Such substance, he thought, would not fall far short of *radiant matter*, by which apparently he meant the supposed caloric or matter assumed to constitute heat, according to the Corpuscular Theory. Even if we could frame the notion, matter in such a state cannot be known to exist, and recent discoveries concerning the continuity of the solid, liquid, and gaseous states remove the basis of the speculation.

From these and many other instances which might be adduced, we learn that analogical reasoning leads us to the conception of many things which, so far as we can ascertain, do not exist. In this way great perplexities have arisen in the use of language and mathematical symbols. All language depends upon analogy; for we join and arrange words so that they may represent the corresponding junctions or arrangements of things and their qualities. But in the use of language we are obviously capable of forming many combinations of words to which no corresponding meaning apparently exists. The same difficulty arises in the use of mathematical signs, and mathematicians have needlessly puzzled themselves about the square root of a negative quantity, which is, in many applications of algebraic calculation, simply a sign without any analogous meaning, there being a failure of analogy.

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[q] 'Principia,' bk. II. Section II. Prop. X.

[r] De Morgan, 'Cambridge Philosophical Transactions,' vol. xi. Part ii. p. 246.

[s] 'Life of Faraday,' vol. i. p. 216.

CHAPTER XXIX.

EXCEPTIONAL PHENOMENA.

IF science consists in the detection of identity and the recognition of one uniformity existing in many objects, it follows almost of necessity that the progress of science depends upon the study of exceptional phenomena. Such new phenomena are the raw material upon which we are to exert our faculties of observation and reasoning, in order to reduce the new facts beneath the sway of the laws of nature, either those laws already well known, or those to be discovered. Not only are strange and inexplicable facts those which are on the whole most likely to lead us to some novel and important discovery, but they are also best fitted to arouse our attention. So long as events happen in accordance with our anticipations, and the routine of every-day observation is unvaried, there is nothing to impress upon the mind the smallness of its knowledge, and the depth of mystery, which may be hidden in the commonest sights and objects. In early times the myriads of stars which remained in apparently fixed relative positions upon the heavenly sphere, received far less notice from astronomers than those few planets whose wandering and inexplicable motions formed an unsolved riddle. Hipparchus was induced to prepare the first catalogue of stars, because a single new star had been added to those nightly visible; and in the mi ages two brilliant but temporary stars caused more popular interest in astronomy than any other events, and to one of them we owe all the observations of Tycho Brahe, the mediæval Hipparchus.

In other sciences, as well as in that of the heavens, exceptional events are commonly the points from which we start to explore new regions of knowledge. It has been beautifully said that Wonder is the daughter of Ignorance, but the mother of Invention; and though the most familiar and slight events, if fully examined, will afford endless food for wonder and for wisdom, yet it is the few peculiar and unlooked-for events which most often lead a scientific mind into a course of discovery. It is true, indeed, that it requires much philosophy to observe things which are too near to us.

The high scientific importance attaching, then, to exceptions, renders it desirable that we should carefully consider the various modes in which an exception may be disposed of; while some new facts will be found to confirm the very laws to which at first sight they seem clearly opposed, others will cause us to limit the generality of our previous statements. In some cases the exception may be proved to be no exception; occasionally it will prove fatal to our previous most confident speculations; and there are some new phenomena which, without really destroying any of our former theories, open to us wholly new fields of scientific investigation. The study of this subject is especially interesting and important, because, as I have before said (vol. ii. p. 233), no important theory can be built up complete and perfect all at once. When unexplained phenomena present themselves as objections to the theory, it will often demand the utmost judgment and sagacity to assign to them their proper place and force. T [illegible] tion or rejection of a theory will entirely [illegible] nating the one insuperable contradictory fact from many, which, however singular and inexplicable at first sight, may afterwards be shown to be results of wholly different causes, or possibly the most striking results of the very law with which they stand in apparent conflict.

I can enumerate at least eight different classes or kinds of exceptional phenomena, to one or other of which any supposed exception to the known laws of nature will ultimately be referred; they may be briefly described as below, and will be sufficiently illustrated in the succeeding sections.

(1) Imaginary, or false exceptions, that is, facts, objects, or events which are not really what they are supposed to be.

(2) Apparent, but congruent exceptions, which, though apparently in conflict with a law of nature, are really in agreement with it.

(3) Singular exceptions, which really agree with a law of nature, but exhibit remarkable and unique results of it.

(4) Divergent exceptions, which really proceed from the ordinary action of known processes of nature, but which are excessive in amount or monstrous in character.

(5) Accidental exceptions, arising from the interference of some entirely distinct but known law of nature.

(6) Novel and unexplained exceptions, which lead to the discovery of a new series of laws and phenomena, modifying or disguising the effects of previously known laws, without being inconsistent with them.

(7) Limiting exceptions, showing the falsity of a supposed law in cases to which it had been extended, but not affecting its truth in other cases.

(8) Contradictory or real exceptions which lead us to the conclusion that a supposed hypothesis or theory is in opposition to the phenomena of nature, and must therefore be abandoned.

It ought to be clearly understood that in no case is a law of nature really thwarted or prevented from being fulfilled. The effects of a law may be disguised and hidden from our view in some instances—in others the law itself may be rendered inapplicable altogether—but if a law is applicable it must be carried out. Every law of nature must therefore be stated with the utmost generality of all the instances really coming under it. Babbage proposed to distinguish between *universal principles*, which do not admit of a single exception, such as that every number ending in 5 is divisible by five, and *general principles* which are more frequently obeyed than violated, as that 'men will be governed by what they believe to be their interest[a].' But in a scientific point of view general principles must be universal as regards some distinct class of objects, or they are not principles at all. If a law to which exceptions exist is stated without allusion to those exceptions, the statement is erroneous. I have no right to say that 'All liquids expand by heat,' if I know that water below 4° C. does not; I ought to say, 'All liquids, except water below 4° C., expand by heat;' and every new exception discovered will falsify the statement until inserted in it. To speak of some laws as being *generally* true, meaning not universally but in the majority of cases, is a hurtful abuse of the word, but is quite usual. *General* should mean that which is true of a whole *genus* or class, and every true statement must be true of some assigned or assignable class.

Imaginary [illegible] Exceptions.

When a s[illegible] law of nature is brought to [illegible] ought properly

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[illegible]

to be—Is there any breach of the law at all? It may be that the supposed exceptional fact is not a fact at all, that it is a mere figment of the imagination. When King Charles requested the Royal Society to investigate the curious fact that a live fish put into a bucket of water does not increase the weight of the bucket and its contents, the Royal Society wisely commenced their deliberations by inquiring whether the fact was so or not. Every statement, however false, must have some cause or prior condition, and the real question for the Royal Society to investigate was, how the King came to think that the fact was so. Mental conditions, as we have seen (vol. ii. p. 4), enter into all acts of observation, and are often a worthy subject of inquiry. But there are many instances in the history of physical science, in which much trouble and temporary error have been caused by false assertions carelessly made, and carelessly accepted without experimental verification.

The reception of the Copernican theory was much impeded by the objection, that if the earth were perpetually moving, a stone dropped from the top of a high tower should be left behind, and should appear to move towards the west, just as a stone dropped from the mast-head of a moving ship would fall behind, owing to the motion of the ship. The Copernicans attempted to meet this grave objection in every way but the true one, namely, that of showing by trial that the asserted facts are not correct ones. In the first place, if a stone had been dropped with suitable precautions from the mast-head of a moving ship, it would have fallen close to the foot of the mast, because by the first law of motion it would remain in the same state of horizontal motion communicated to it b
mast. As the anti-Copernicans had assume
result as certain to ensue, their argument
have fallen through at once. Had the

proceeded to test with great care the other assertion involved, they would have become still better convinced of the truth of their own theory. A stone dropped from the top of a high tower, or into a deep well, would certainly not have been deflected from the vertical direction in the considerable degree required to support the anti-Copernican views; but, with very accurate observation, they might have discovered, as Benzenberg subsequently did, a very small deflection towards the west (vol. i. p. 453). At the moment when a body begins to fall freely, it begins to resemble a very small satellite moving under the force of gravity, as exerted from the earth's centre of attraction, and it therefore describes, like other satellites, a portion of an elliptic orbit[b]. Had the Copernicans then been able to detect and interpret the meaning of this small divergence, they would have found in it a conclusive proof of their own views.

Multitudes of cases might be cited in which laws of nature seem to be evidently broken, but in which the apparent breach entirely arises from a misapprehension of the facts of the case. It is a general law, absolutely true of all crystals yet submitted to examination, that no crystal has a re-entrant angle, that is an angle which towards the axis of the crystal is greater than two right angles. Wherever the faces of a crystal meet they produce a projecting edge, and wherever edges meet they produce a corner. Many crystals, however, when carelessly examined, present exceptions to this law, but closer observation always shows that the apparently re-entrant angle really arises from the oblique union of two distinct crystals. Other crystals seem to possess faces contradicting al[illegible] principles of crystallography; but again [illegible] that the supposed faces are not forms are similarly modified, so as to produce a series of what are called *hemihedral* forms.

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[b] [illegible] matical Journal' (1848), vol. iii.

Apparent but Congruent Exceptions.

Not unfrequently a law of nature will present results in certain circumstances which appear to be entirely in conflict with the law itself. Not only may the action of the law be much complicated and disguised, but it may in various ways be reversed or inverted, so that all careless observers are misled. Ancient philosophers generally believed that while some bodies were heavy by nature, others, such as flame, smoke, bubbles, clouds, &c., were essentially light, or possessed a tendency to move upwards. So acute and learned an inquirer as Aristotle entirely failed to perceive the true nature of buoyancy or apparent lightness, and the doctrine of intrinsic lightness, being expounded in his works, became the accepted view for many centuries. It is true that Lucretius was fully aware why flame tends to rise, holding that—

> 'The flame has weight, though highly rare,
> Nor mounts but when compelled by heavier air.'

Archimedes also was so perfectly acquainted with the buoyancy of bodies immersed in water, that he could not fail to perceive the existence of a parallel effect in air. Yet throughout the early middle ages the light of true science, clear though feeble, could not contend with the powerful but confused glare of the false Peripatetic doctrine. The genius of Galileo and Newton was required to convince people of the simple truth that all matter is heavy, but that the gravity of one substance may be overborne by that of another, as one scale of a balance is carried up by the preponderating weight in the opposite scale. It is curious to find Newton gravely explaining the difference of absolute and relative gravity, as if it were a new discovery proceeding from his theory[e]. More than a century elapsed before other apparent exceptions to the Newtonian philosophy were explained away.

Newton himself allowed that the motion of the apsides of the moon's orbit appeared irreconcilable with the law of gravity, and it remained for Clairaut to remove the reproach by more complete mathematical analysis. There must always indeed remain, in the motions of the tides or of the heavenly bodies, discrepancies of some amount between theory and observation; but like discrepancies have so often yielded in past times to prolonged investigation that all physicists have come to regard them as merely apparent exceptions, which will afterwards be found to be new confirmations of the law with which they now seem to conflict.

The most beautiful instance, perhaps, which can be adduced of an apparent exception, is found in the total reflection of light, which occurs when a beam of light within a medium falls very obliquely upon the boundary separating it from a rarer medium. It is the general law that when a ray strikes the limit between two media of different refractive indices, part of the light is reflected and part is refracted, but when the obliquity of the ray within the denser medium passes beyond a certain point there is a sudden apparent breach of continuity, and the whole of the light is reflected. A very clear reason can be given for this exceptional conduct of the light; for according to the law of refraction the sine of the angle of incidence always bears a fixed ratio to the sine of the angle of refraction, so that the greater of the two angles, which is always that in the less dense medium, may increase up to a right angle, but when the media differ in refractive power, the less angle cannot become a right angle, as this would require the sine of an angle to be greater than the radius. It might seem, perhaps, that this was an exception of the kind elsewhere described as a limiting exception, in which a law is shown to be inapplicable beyond certain defined limits; but in the explanation of the exception according to the undulatory theory, we find that there is really no breach or exception to the general law. Whenever an undulation strikes any point in a bounding surface, spherical waves are produced and spread from the point. The refracted ray is the resultant of an infinite number of such spherical waves, and the bending of the ray at the common surface of two media depends upon the comparative velocities of propagation of the undulations in those media. But if a ray falls very obliquely upon the surface of a rarer medium, the waves arising from successive points of the surface may spread so rapidly as never to intersect, and no resultant wave will then be produced. We thus perceive that from general mathematical conditions may arise very distinct apparent effects.

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[e] 'Principia,' bk. II. Prop. 20. Corollaries, 5 and 6.

There may occur from time to time distinct failures in our most well-grounded predictions. A comet, of which the orbit has been well determined, may fail, like Lexell's Comet, to appear at the appointed time and place in the heavens. In the present day we should not hold such an exception to our successful predictions to weigh against our belief in the theory of gravitation, but should assume that some unknown body had through the action of gravitation itself deflected the comet. As Clairaut remarked, in publishing his calculations concerning the expected reappearance of Halley's Comet, a body which passes into regions so remote, and which is hidden from our view during such long periods, might be exposed to the influence of forces totally unknown to us, such as the action of other comets, or even of some planet too far removed from the sun to be ever perceived by us. In the case of

Lexell's Comet it was afterwards shown, curiously enough, that its appearance was not one of a regular series of periodical returns within the sphere of our vision, but a single exceptional visit never to be repeated, and probably due to the perturbing powers of Jupiter. Yet this solitary visit was a strong confirmation of the law of gravity with which it seemed to be in conflict.

The division of Biela's Comet into two companion comets was at the time when it occurred one of those unlooked-for and inexplicable events which awaken the attention and interest of observers in the highest degree. Comets indeed have altogether the character of eccentric strangers intruding into our planetary system, and in almost every point they are yet inexplicable ; but there is a possibility that the separation of Biela's Comet may prove to be a comparatively ordinary event of cometary history. For if, as is now believed, comets be aggregates of small meteoric stones or particles, forming the denser parts of continuous streams of such bodies circulating round the sun, then it is not unlikely that these aggregates may at times be increased or diminished by the meeting or separation of meteoric streams.

Singular Exceptions.

Among the most interesting of apparent exceptions are those which I propose to call *singular exceptions*, because they are more or less closely analogous to the singular cases, or solutions which occur in mathematical science. A general mathematical law embraces an infinite multitude of cases which have a perfect agreement with each other in a certain respect. It may nevertheless happen that a single case, while obeying the general law, stands out as apparently different from all the rest. The daily rotation of the earth upon its axis gives to all the stars in the heavens an apparent relative motion of rotation from east to west; but out of countless thousands which obey the rule the Pole Star alone seems to break it. Exact observations indeed show that it also revolves in a small circle, but it might happen for a short time that a star existed so close to the pole that no appreciable change of place would be caused by the daily rotation. It would then constitute a perfect singular exception; for, while really obeying the law, it would break the terms in which it is usually stated. In the same way the poles of every revolving body are singular points.

Whenever the laws of nature are reduced to a mathematical form we may expect to meet with singular cases, and, as all the physical sciences will meet in the mathematical principles of mechanics, there is no part of nature where we may not probably encounter them. In mechanical science itself the circular motion of rotation may be considered a single exception to the rectilineal motion of translation. It is a general law that any number of parallel forces, whether acting in the same or opposite directions, will have a resultant which may be substituted for them with like effect. This resultant will be equal to the algebraic sum of the forces, or the difference of those acting in one direction and the other; it will pass through a point which is determined by a simple formula, and which may be described as the mean point of all the points of application of the parallel forces (vol. i. p. 422). Thus we readily determine the resultant of parallel forces, except in one peculiar case, namely, when two forces are equal and opposite but not in the same straight line. Being equal and opposite the amount of the resultant is nothing, yet, as the forces are not in the same straight line, they do not balance and destroy each other. Examining the formula for the point of application of the resultant, we find that it gives an infinitely great magnitude, so that the resultant is nothing at all, and acts at an infinite distance, which is practically the same as to say that there is no possible single resultant. Two such forces constitute what is known in mechanical science as a *couple*, which occasions rotatory instead of rectilineal motion, and can only be neutralized by an equal and opposite couple or pair of forces.

The most beautiful instances of singular exceptions are furnished by the science of optics. It is a general law, for instance, that in passing through transparent media the plane of vibration of polarized light remains unchanged. But in certain cases, to which reference has already been frequently made, namely, certain liquids, some peculiar crystals of quartz, and transparent solid media subjected to a magnetic strain, as in Faraday's experiment (vol ii. pp. 234, 287), the plane of polarization is rotated in a screw-like manner. This effect is so entirely *sui generis*, so unlike any other phenomena in nature, as to appear truly exceptional; yet mathematical analysis shows it to be only a single case of much more general laws. As stated by Thomson and Tait[f], it arises from the composition of two uniform circular motions. If while a point is moving round a circle, the centre of that circle move upon another circle, a great variety of curious curves will be produced according as we vary the dimensions of the circles or the rapidity of the motions. In one case where the two circles are exactly equal, the point will be found to move gradually round the centre of the stationary circle, and describe a curious star-like figure connected with the molecular motions out of which the rotational power of the media arises. Among other singular exceptions in optics may be placed the conical refraction of light, already noticed (vol. ii. p. 175), connected with the peculiar form assumed by a wave of light when passing through certain double-refracting crystals. The laws obeyed by the wave are exactly the same as in other cases, yet the results are entirely *sui generis*. So far are such cases from contradicting the theory of ordinary cases, that they afford the supreme opportunities for verification.

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[f] 'Treatise on Natural Philosophy,' vol. i. p. 50.

In astronomy singular exceptions might occur, and in an approximate manner they do occur. We might point to the rings of Saturn as objects which, though undoubtedly obeying the law of gravity, are yet entirely unique, as far as our observation of the universe has gone. They agree, indeed, with the other bodies of the planetary system in the stability of their movements, which never diverge far from the mean position. But a truly singular event might happen, or might have happened, under slightly different circumstances. Had the rings been exactly uniform all round, and with a centre of gravity coinciding for a moment with that of Saturn, a singular case of unstable equilibrium would have arisen, necessarily resulting in the sudden collapse of the rings, and the fall of their debris upon the surface of the planet. Thus in one single case the theory of gravity would give a result wholly unlike anything else known in the mechanism of the heavens.

It is possible that we might meet with singular exceptions in crystallography. If a crystal of the second or dimetric system, in which the third axis is usually unequal to either of the other two, happened to have the three axes equal, it might be mistaken at first sight for a crystal of the cubic system, but would in many ways exhibit different faces and dissimilar properties. There is, again, a possible class of diclinic crystals in which two axes are at right angles and the third axis inclined to the other two. This class is chiefly remarkable for its non-existence in [illegible] t of view, since no crystals have yet been proved to have such axes. It seems likely that the class would constitute only a singular case of the more general triclinic system, in which all three axes are inclined to each other at various angles. Now if the diclinic form were merely accidental, and not necessitated by any general law of molecular constitution, its actual occurrence would be infinitely improbable, just as it is infinitely improbable that any star should indicate the North Pole with perfect exactness.

In the curves denoting the relation between the temperature and pressure of water there is one very remarkable point entirely single and unique, at which alone water can remain in the three conditions of gas, liquid, and solid in the same vessel. It is the point at which three curves intersect, namely, the steam line showing at what temperatures and pressures water is just upon the point of becoming gaseous, and other similar lines which show when ice is just on the point of melting, and when ice is just about to assume the gaseous state directly.

Divergent Exceptions.

Closely analogous to singular exceptions are those divergent exceptions, in which a phenomenon manifests itself in very unusual magnitude or character, without however in any degree becoming subject to peculiar laws. Thus in throwing ten coins, it happened in four cases out of 2048 throws, that all the coins fell with heads uppermost (vol. i. p. 238); these would usually be regarded as very singular events, and, according to the theory of probabilities, they would be comparatively very rare; yet they proceed only from an unusual conjunction of accidental events, and from no really exceptional causes. In all classes of natural phenomena we may expect to meet with similar divergencies from the average. Sometimes due merely to the principles of probability, sometimes to deeper reasons. Among every large collection of persons, we shall probably find some persons who are remarkably large or remarkably small, giants or dwarfs, whether in bodily or mental conformation. Such cases appear to be not mere *lusus naturæ*, since they usually occur with a frequency closely accordant with the law of error or divergence from an average, as shown by M. Quetelet and Mr. Galton (vol. i. p. 446). The rise of genius, or the occurrence of extraordinary musical or mathematical faculties, are attributed by M. Galton to the same principle of divergence.

Under this class of exceptions I am inclined to place all kinds of remarkable events arising from an unusual conjunction of many ordinary tendencies. When several distinct forces happen to concur together, we may have surprising or alarming results. Great storms, floods, droughts and other extreme deviations from the average condition of the atmosphere thus arise. They must be expected to happen from time to time, and will yet be very unfrequent compared with minor disturbances. They are not anomalous but only extreme events, exactly analogous to extreme runs of luck. There seems, indeed, to be a fallacious impression in the minds of many persons, that the theory of probabilities necessitates uniformity in the happening of events, so that in the same space of time there will always be closely the same number, for instance, of railway accidents and murders. Buckle has superficially remarked upon the comparative constancy of many such events as ascertained by Quetelet, and some of his readers acquire the false notion that there is a kind of mysterious inexorable law producing uniformity in natural and human affairs. But nothing can be more opposed to the teachings of the theory of probability, which always contemplates the occurrence of extreme and unusual runs of luck. That theory shows the great improbability that the number of railway accidents per month should be always equal, or nearly so. The public attention is strongly attracted to any unusual conjunction of events, but there is a fallacious tendency to suppose that every such conjunction must be due to a peculiar new cause coming into operation. Unless it can be clearly shown that such unusual conjunctions occur more frequently than they should do according to the theory of probabilities, we should regard them as merely divergent exceptions.

Eclipses and remarkable conjunctions of the heavenly bodies may also be regarded as results of ordinary laws, which nevertheless appear to break the regular course of nature, and never fail to excite surprise or even fear. Such conjunctions of bodies vary greatly in frequency. One or other of the satellites of Jupiter is eclipsed almost every day, but the simultaneous eclipse of three satellites can only take place, according to the calculations of Wargentin, after the lapse of 1,317,900 years. The relations of the four satellites are so remarkable, that it is actually impossible, according to the theory of gravity, that they should all suffer eclipse simultaneously. But it may happen occasionally that while some of the satellites are really eclipsed by entering Jupiter's shadow, the others are either occulted or rendered invisible by passing over his disk, as seen by us. Thus on four occasions, in 1681, 1802, 1826, and 1843, Jupiter has been witnessed in the singular condition of being apparently deprived of satellites. A close conjunction of two planets always excites surprise and admiration, though conjunctions must naturally occur at intervals in the ordinary course of their motions. We cannot wonder, then, that when three or four planets approach each other closely, the event is long remembered. A most exceptional conjunction of Mars, Jupiter, Saturn, and Mercury, which took place in the year 2446 B.C., was adopted by the Chinese Emperor, Chuen Hio, as a new epoch for the chronology of that Empire, though there is some doubt whether the conjunction was really observed or was calculated from the supposed laws of motion of the planets. It is certain that on the 11th November, 1524, the planets Venus, Jupiter, Mars, and Saturn were seen very close together, while Mercury was only distant by about 16° or thirty apparent diameters of the sun, this conjunction being probably the most remarkable which has occurred in historical times.

Among the perturbations of the planetary motions we may find divergent exceptions arising from the peculiar accumulation or intensification of effects, as in the case of the long inequality of Jupiter and Saturn (vol. ii. p. 70). Leverrier has shown that there is one place between the orbits of Mercury and Venus, and another between those of Mars and Jupiter, in either of which, if a small planet happened to exist, it would suffer comparatively immense disturbance in the elements of its orbit. Now between Mars and Jupiter there do occur the minor planets, the orbits of which are in many cases exceptionally divergent[g].

It is worthy of notice that even in such a subject as formal logic, divergent exceptions seem to occur, not of course due to chance, but exhibiting in an unusual degree a phenomenon which is more or less manifested in all other cases. I pointed out in p. 162 of the first volume, that propositions of the general type $A = BC + bc$ are capable of expression in six equivalent logical forms, so that they manifest in a higher degree than any other proposition yet discovered, the phenomenon of logical equivalency.

Under the head of divergent exceptions we might doubtless place all or nearly all of the instances of substances possessing physical properties in a very high or low degree, which were described in the chapter on

[g] Grant's 'Hist[illegible]my,' p. 116.

Generalization, (vol. ii. p. 259). Quicksilver is divergent among metals as regards its melting point, and potassium and sodium as regards their specific gravity. Monstrous productions and variations, whether in the animal or vegetable kingdoms, should probably be assigned to this class of exceptions.

Accidental Exceptions.

The third and largest class of exceptions contains those which arise from the casual interference of extraneous causes. A law may be in operation, and, if so, must be perfectly fulfilled, but, while we conceive that we are examining its results, we may have before us the effects of a totally different cause, possessing no connexion with the subject of our inquiry. The law is not really broken, but at the same time the supposed exception is not illusory. It may be a phenomenon which cannot occur but under the condition of the law in question, yet there has been such subsequent interference and modification of the result, that there is an apparent failure of science. There is, for instance, no subject in which more rigorous and invariable laws have been established than in crystallography. As a general rule, each chemical substance possesses its own definite form, by which it can be infallibly recognised; but the mineralogist has to be on his guard against what are called *pseudomorphic* crystals. In some circumstances a substance, having perfectly assumed its proper crystalline form, may afterwards undergo chemical change; a new ingredient may be added, a former one removed, or one element may be substituted for another. In carbonate of lime the carbonic acid is sometimes replaced by sulphuric acid, so that we find gypsum in the form of calcite; other cases are known where the change is inverted and calcite [illegible]e form of gypsum. Mica, talc, steatite, [illegible]inerals subject to these curious transmutations. Sometimes a crystal embedded in a matrix is entirely dissolved away, and subsequently a new kind of mineral is gradually deposited in the cavity as in a mould. Quartz is thus found cast in many forms wholly unnatural to it. A still more perplexing case sometimes occurs. Carbonate of lime is one of the substances capable of assuming two distinct forms of crystallization, in which it bears respectively the names of calcite and arragonite. Now arragonite, while retaining its outward form unchanged, may undergo an internal molecular change into calcite, as indicated by the altered cleavage. Thus we may come across crystals apparently of arragonite, which seem to break all the laws of crystallography, by possessing the cleavage of an entirely different system of crystallization.

Some of the most invariable and certain laws of nature are disguised by interference of unlooked-for causes. While the barometer was yet a new and curious subject of investigation, its theory, as stated by Torricelli and Pascal, seemed to be contradicted by the fact that in a well-constructed instrument the mercury would often stand far above 31 inches in height. Boyle showed[h] that the mercury could be made to rise as much as 75 inches in a perfectly cleansed tube, or about two and a half times as high as could be due to the pressure of the atmosphere. Many absurd theories about the pressure of imaginary fluids were in consequence put forth[i], and the subject was involved in much confusion until the adhesive or cohesive force between glass and mercury, when brought into perfect contact, was pointed out as the real interfering cause.

Guy-Lussac, again, observed that the temperature of boiling water was very different in some kinds of vessels from what it was in others. It is only in contact with metallic surfaces or sharply broken edges that the temperature is at all fixed at 212° Fahr. The suspended freezing of liquids is another case where the action of a law of nature appears to be interrupted. Spheroidal ebullition seemed at first sight a most anomalous phenomenon; it was almost incredible that water should not boil in a red-hot vessel, or that ice could actually be produced in a red-hot crucible. These paradoxical results are now fully explained as due to the interposition of a non-conducting film of vapour between the globule of liquid and the sides of the vessel. The feats of conjurors who handle liquid metals are readily accounted for in the same manner. At one time the *passive state* of steel was regarded as entirely anomalous. It may be assumed as a general law that when two pieces respectively of electro-negative and electro-positive metal are placed in nitric acid, and made to touch each other, the electro-negative metal will undergo rapid solution. But when iron is the electro-negative and platinum the electro-positive, the solution of the iron entirely and abruptly ceases. Faraday ingeniously proved that this effect was due to a thin film of oxide of iron, which forms upon the surface of the iron and protects it[k].

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[h] 'Discourse to the Royal Society,' 28th May, 1684.

[i] Robert Hooke's 'Posthumous Works,' p. 365.

The law of gravity is of so simple and general a character, and is apparently so disconnected from the other laws of nature, that it never suffers any disturbance, and is in no way disguised, but by the complication of its own effects. It is otherwise, however, with those entirely secondary laws of the planetary system, which have only an empirical basis. The fact that all the long known planets and satellites have a similar motion from west to east is not necessitated by any principles of science, but points merely to some common condition existing in the nebulous mass from which our system has doubtless been evolved. The retrograde motions of the satellites of Uranus constituted a distinct breach in this law of uniform direction, which became all the more interesting when the single satellite of Neptune was also found to be retrograde. It now became probable, as Baden Powell well observed, that the anomaly would cease to be singular, and become a case of another law, pointing to some general interference, which has taken place on the bounds of the planetary system. Not only have the satellites suffered from this perturbance, but Uranus is also anomalous in having an axis of rotation lying nearly in the ecliptic; and Neptune constitutes a distinct exception to the empirical law of Bode concerning the distances of the planets, which exceptional circumstance may possibly be due to the same disturbance.

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[k] 'Experimental Researches in Electricity,' vol. ii. pp. 240–245.

Geology is a science in which accidental exceptions are very likely to occur. Only when we find strata in their original relative positions, can we surely infer that the order of succession is the order of time. But it not uncommonly happens that strata are inverted by the bending and doubling action of extreme pressure. Landslips may carry one body of rock into proximity with an unrelated series, and produce results apparently inexplicable[1]. Floods, streams, icebergs, and other casual agents, may occasionally lodge remains in places where they would be wholly unexpected.

Though such interfering causes may have been often wrongly supposed to explain important discoveries, the geologist must of course always bear the possibility of interference in mind. Scarcely more than a century ago it was yet held by many persons that fossils were accidental productions of nature, mere forms into which minerals had been shaped by no peculiar cause. Voltaire appears not to have been able to accept such an explanation; but fearing that the occurrence of fossil fishes on the Alps would support the Mosaic account of the deluge, he did not hesitate to attribute them to the remains of fishes accidentally brought there by travellers or pilgrims. In archæological investigations the greatest caution is requisite in allowing for secondary burials in ancient tombs and tumuli, for imitations, casual coincidences, disturbance by subsequent races, or even by other archæologists, in fact, for a multitude of interfering circumstances. In common life extraordinary events must happen from time to time, as when a shepherdess in France was astonished at an iron chain falling out of the sky near to her feet, the fact being that Guy-Lussac had thrown it out of his balloon, which was passing over her head unseen at the time.

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[1] Murchison's 'Silurian System,' vol. ii. p. 733, &c.

To this class of accidental exceptions I would refer the innumerable breaches of the rules of inflexion in grammar. These rules would be invariable were it not that the forms derived from distinct roots sometimes get mixed together, that mistaken analogies sometimes occasion confusion, and a variety of such disturbing causes produce irregularity. Philology already presents beautiful instances of the manner in which a comprehensive law may be traced out in a thoroughly scientific manner, in spite of apparently inexplicable exceptions.

Novel and Unexplained Exceptions.

When a law of nature appears to fail because some other law has interfered with its action, two cases may obviously present themselves;—the interfering law may be a known and familiar one, or it may have been previously undetected. In the first case, which we have sufficiently considered in the preceding section, we have nothing to do but calculate as exactly as possible the amount of interference, and make allowance for it; the apparent failure of the law under examination should then disappear. But in the second case the results may be much more important. A phenomenon which entirely fails to be explained by any known laws may indicate the interference of some wholly new series of natural forces. The ancients could not help perceiving that the general tendency of bodies downwards failed in the case of the loadstone, nor would the doctrine of essential lightness explain the exception, since the substance drawn upwards by the loadstone is a heavy metal. We now see clearly that there was no breach in the perfect generality of the law of gravity, but that a new form of energy manifested itself in a conspicuous form in the loadstone for the first time. In this case the forces concerned, those of gravity and electrical attraction, have never yet been brought into correlation with each other.

Other sciences show us that laws of nature, rigorously true and exact, may often be developed by those who are ignorant of far more complex phenomena involved in their application. Newton's comprehension of geometrical optics was sufficient to explain all the ordinary refractions and reflections of light. The simple laws of the bending of rays apply to all rays, whatever the character of the undulations composing them. Newton suspected the existence of other classes of phenomena when he spoke of rays as *having sides;* but it remained for later experimentalists to show that light is a transverse undulation, like the bending of a rod or cord.

Dalton's atomic theory is doubtless true of all chemical compounds, and the essence of it is that the same compound will alwa [illegible] l to contain the same elements in certain [illegible] s. Pure calcium carbonate contains [illegible] ygen to 40 of calcium, and 12 of carbon. But when careful analyses were made of a great many minerals, this law often appeared to fail. What was unquestionably the same mineral, judging by its crystalline form and physical properties, would often give varying proportions of its components, and would sometimes contain unusual elements which yet could not be set down as mere impurities. Dolomite, for instance, is a compound of the carbonates of magnesia and lime, but specimens from different places do not exhibit any fixed ratio between the lime and magnesia, and carbonate of iron occasionally forms a real constituent of the mineral. Such facts could be reconciled with the laws of Dalton only by supposing the interference of a new law, that of Isomorphism.

It is now sufficiently established that certain elements are closely related to each other, so that they can, as it were, step into each other's places without apparently altering the form of the compound molecules, or the shape of the crystals which they constitute. The carbonates of iron, calcium, and magnesium, are nearly identical in their crystalline forms, hence they may crystallize together in harmony, producing mixed minerals of considerable complexity, which nevertheless perfectly verify the laws of equivalent proportions. This principle of isomorphism once established, not only explains what was formerly a stumbling-block, but gives most valuable aid to chemists in deciding upon the real constitution of new salts, since those compounds of isomorphous elements which have identical crystalline forms must possess corresponding chemical formulæ.

We may always expect that from time to time new and extraordinary phenomena will be discovered, and will lead to new views of the laws of nature. The recent observation, for instance, that the resistance of a bar of selenium to a current of electricity is affected in an extraordinary degree by rays of light falling upon the selenium, points to a wholly new relation between light and electricity. The peculiar so-called allotropic changes which sulphur, selenium, and phosphorus undergo by an alteration in the amount of latent heat which they contain, will probably lead at some future time to important inferences concerning the molecular constitution of solids and liquids. The curious substance ozone has perplexed many chemists, and Andrews and Tait thought that it afforded evidence of the decomposition of oxygen by the electric discharge. The researches of Sir B. C. Brodie negative this notion, and afford evidence of the real constitution of the substance[m], which still, however, remains exceptional in its properties and relations, and affords a hope of important discoveries in chemical theory.

Limiting Exceptions.

We may pass to cases where exceptional phenomena are actually irreconcilable with a law of nature previously regarded as true by philosophers. Error must now be allowed to have been committed, but it is obvious that the error may be more or less extensive. It may be that a law holding rigorously true of the facts actually under notice had been extended by generalization to other series of facts then unexamined. Subsequent investigation may show the falsity of this generalization, and the result must be to limit the law for the future to those objects of which it is really true, while we bring the other classes of objects under distinct generalizations. The contradiction to our previous opinions is partial and not total.

Newton laid down as a result of experiment that every ray of homogeneous light has a definite refrangibility, which it preserves throughout its course until extinguished. This is indeed but one case of the general principle of undulatory movement, which Sir John Herschel has stated in the most complete manner under the title, 'Principle of Forced Vibrations' (vol. ii. p. 65), and has asserted to be absolutely universal and without exception. But Sir John Herschel himself described in the 'Philosophical Transactions' for 1845 a curious appearance in a solution of quinine; as viewed by transmitted light the solution appeared colourless, but in certain aspects it possessed a beautiful celestial blue tint. Curiously enough the coloured light comes only from the first portion of liquid which the light enters. Similar phenomena in fluor-spar had been described by Sir D. Brewster in 1838. Professor Stokes, having minutely investigated the phenomena, discovered that they were more or less present in almost all vegetable infusions, and in a number of mineral substances. He came to the conclusion that this phenomenon, called by him Fluorescence, could only be explained by a degradation or alteration in the refrangibility of the rays of light; he asserts, in fact, that light-rays of very short length of vibration in falling upon certain atoms excite undulations of greater length, in total opposition to the principle of forced vibrations. No complete explanation of the mode of change is yet possible, because it evidently depends upon the intimate constitution of the atoms of the substances concerned; but Professor Stokes believes that the principle of forced vibrations is true only so long as the excursions of an atom are very small compared with the magnitude of the complex molecules[n]. It is now also well known that in Calorescence the refrangibility of rays may be increased and the wave-length diminished. Rays of obscure heat and low refrangibility may be concentrated so as to heat a solid substance, and make it give out rays belonging to any part of the spectrum, and it seems probable that this effect arises from the impact of distinct but conflicting atoms. Nor is it in light only that we discover limiting exceptions to the law of forced vibrations ; for if we closely observe gentle waves lapping upon the stones at the edge of a lake or other piece of water, we shall notice that each larger wave in breaking upon a stone gives rise to a series of waves of a smaller order. Thus there must be constantly in progress a degradation in the magnitude of water-waves. The principle of forced vibrations seems then to be too generally stated by Sir John Herschel, but it must be a very difficult question of mechanical theory to discriminate the circumstances in which it does and does not hold true.

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[m] 'Philosophical Transactions' (1872), vol. clxii. no. 23.

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[n] 'Philosophical Transactions' (1852), vol. cxlii. pp. 465, 548, &c.

We may sometimes foresee the possible existence of exceptions yet unknown by experience, and limit the statement of our discoveries accordingly. Very extensive inquiries have shown that all substances yet examined fall into one of two classes ; they are all either ferromagnetic, that is, magnetic in the same way as iron, or they are diamagnetic like bismuth. But it does not thence follow that every substance must be ferro-magnetic or diamagnetic. The magnetic properties are shown by Sir W. Thomson[o] to depend upon the specific inductive capacities of the substance in three rectangular directions. If these inductive capacities are all positive, we have a ferro-magnetic substance ; if negative, a diamagnetic substance ; but if the specific inductive capacity were positive in one direction and negative in the others, we should have an exception to previous experience, and could not place the substance under either of the present recognised classes.

So many gases have been reduced to the liquid state, and so many solids fused ic men rather hastily adopted the generalization that all substances could exist in all three states. A certain number of gases, such as oxygen, hydrogen, and nitrogen, have resisted all efforts to liquefy them, and it now seems probable from the experiments of Dr. Andrews that they are limiting exceptions. Dr. Andrews finds that above 88° Fahr. carbonic acid cannot be liquefied by any pressure he could apply, whereas below this temperature liquefaction is always possible. By analogy it becomes highly probable that even hydrogen might be liquefied if cooled to a sufficiently low temperature. We must modify our previous views, and either assert that *below a certain critical temperature* every gas may be liquefied, or else we must assume that a highly condensed gas is, when above the critical temperature, undistinguishable from a liquid. At the same time we receive an explanation of a remarkable exception presented by liquid carbonic acid to the general rule that gases expand more by heat than liquids. This liquid carbonic acid was found by Thilorier in 1835 to expand more than four times as much as air; but by the light of Dr. Andrews' experiments we may learn to regard the liquid as rather a highly condensed gas than an ordinary liquid, and it is actually possible to reduce the gas to the apparently liquid condition without any abrupt condensation[p].

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[o] 'Philosopl ' p. 182.

It is an empirical law of the planetary system that all the bodies composing it revolve from west to east; that law is broken, as we have seen, in the cases of one planet and several satellites, probably by the interference of an accidental disturbing force. The law also fails to be true of comets, which, taken as a whole, appear to move according to no single uniform law. This exception, however, is one of limitation only, for in all probability comets, although at present members of our system, have not always been so, but have, in wandering through space, been entangled in our system and retained by the attractive influence of Jupiter, or one of the other larger planets. We must then limit the statement of the law of uniform direction to bodies which are derived from the original constituents of the nebulous mass.

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[p] Maxwell, 'Theory of Heat,' p. 123.

Limiting exceptions occur most frequently in the natural sciences of Botany, Zoology, Geology, &c., the laws of which are almost wholly empirical. In innumerable instances the confident belief of one generation has been falsified by the wider observation of a succeeding one. Aristotle confidently held that all swans are white[q], and the proposition seemed true until not a hundred years ago black swans were discovered in Western Australia. At one time all the animal remains discovered in the Scottish Old Red Sandstone were fishes or shells, until at last a single small air-breathing reptile occurred opportunely to prevent any hasty conclusions[r]. In zoology and physiology we may expect a fundamental identity to exist in the vital processes, but continual discoveries show that there is no limit to the apparently anomalous expedients by which life is reproduced. Alternate generation, fertilization for several successive generations, hermaphroditism, are opposed to all we should expect from induction founded upon the higher animals. But such phenomena are only limiting exceptions showing that what is true of one class is not true of another. In certain of the cephalopoda we meet the extraordinary fact that an arm of the male is cast off and lives independently until it encounters the female.

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[q] 'Prior Analytica,' ii. 2, 8, and elsewhere.

[r] Murchison's 'Siluria' (1854), p. 254.

Real Exceptions to Supposed Laws.

The exceptions which we have lastly to consider, are perhaps the most important of all, since they lead to the entire rejection of a law or theory before accepted. No law of nature can fail; there are no such things as real exceptions. Where contradiction exists it must be in the mind of the experimentalist. Either the law is imaginary or the phenomena which conflict with it; if, then, by our senses we can satisfy ourselves of the actual occurrence of the phenomena, the law must be rejected as illusory. The followers of Aristotle held that nature abhorred a vacuum, and thus accounted for the rise of water in a pump. When Torricelli pointed out the visible fact that water would not rise more than 33 feet in a pump, nor mercury more than about 30 inches in a glass tube, they attempted to represent these facts as limiting exceptions, saying that nature abhorred a vacuum to a certain extent and no further. But the Academicians del Cimento completed their discomfiture by showing that if we remove the pressure of the surrounding air, and in proportion as we remove it, nature's feelings of abhorrence decrease and finally disappear altogether. Even Aristotelian doctrines could not stand such direct contradiction.

Lavoisier's ideas concerning the constitution of acids received complete refutation. He named oxygen the *acid generator*, because he believed that all acids were compounds of oxygen, a generalization based on insufficient data. Berthollet, as early as 1789, proved by analysis that hydrogen sulphide and prussic acid, both clearly acting the part of acids, were devoid of oxygen; the former might perhaps have been interpreted as a limiting exception, but when so powerful an acid as hydrogen chloride (muriatic acid) was found to contain no oxygen the theory had to be relinquished. Berzelius' theory of the dual formation of chemical compounds has met a similar fate.

It is obvious that all conclusive *experimenta crucis* constitute real exceptions to the supposed laws of the theory which is overthrown. Newton's corpuscular theory of light was not rejected on account of its absurdity or inconceivability, for in these respects it is, as we have seen, far superior to the undulatory theory. It was rejected because certain small diffraction fringes of colour did not appear in the exact place and of the exact size which calculation showed that they ought to appear according to the conditions of the theory (vol. ii. pp. 145–151). One single fact clearly irreconcilable with a theory involves its total rejection. In the greater number of cases, what appears to be a fatal exception, may be afterwards explained away as a singular or disguised result of the very laws with which it seems to conflict, or as due to the interference of extraneous causes; but if we fail thus to reduce the fact to congruity, it remains more powerful than any theories or any dogmas.

Of late years not a few of the favourite doctrines of geologists have been rudely destroyed. It was the general belief that human remains were to be found only in those deposits which are actually in progress at the present day, so that the creation of man appeared to have taken place at the beginning, as it were, of this geological age. The discovery of a single worked flint in older strata and in connexion with the remains of extinct mammals was sufficient to explode such a doctrine. Similarly, the opinions of geologists have been altered by the discovery of the Eozoön in the L [illegible] Canada; it was previously held th [illegible] curred in any older strata than [illegible] As the examination of the strata of the globe becomes more and more complete, our views of the origin and succession of life upon the globe must undergo many changes and extensions.

Unclassed Exceptions.

At every period of scientific progress there will necessarily exist a multitude of exceptional and unexplained phenomena which we know not how to regard. They are the outstanding facts upon which the labours of investigators must be exerted,—the ore from which the gold of future discovery is to be extracted. It might be thought that, as our knowledge of the laws of nature increases, the number of such exceptions should decrease; but, on the contrary, the more we know the more there is yet to learn and explain. This arises from several reasons; in the first place the principal laws and forces in nature are numerous, so that he who bears in mind the wonderfully large numbers developed in the doctrine of combinations, will anticipate the existence of almost infinitely numerous relations of one law to another. When we are once in possession of a law, we are potentially in possession of all its consequences; but it does not follow that the mind of man, so limited in its powers and capacities, can actually work them all out in detail. Just as the aberration of light was discovered empirically, though it should have been foreseen, so there are doubtless multitudes of unexplained facts, the connexion of which with laws of nature already known to us, we should perceive, were we not hindered by the imperfection of our deductive powers. But, in the second place, as will be more fully pointed out, it is not to be supposed that we have in any degree approximated to an exhaustion of nature's powers. The most familiar facts may teem with indications of forces, now secrets hidden from us, because we have not mind-directed eyes to discriminate them. The progress of science will consist in the discovery from time to time of new exceptional phenomena, and their assignment by degrees to one or other of the heads already described. When a new fact proves to be merely a false, apparent, singular, divergent, or accidental exception, we may gain a more minute and accurate acquaintance with the effects of certain laws already known to exist. We have indeed no addition to what was implicitly in our possession, but, as already explained, there is much difference between knowing the laws of nature and perceiving all their complicated effects. Should a new fact prove to be a limiting or real exception, we have to alter, in part or in whole, our views of nature and are saved from errors into which we had fallen. Lastly, the new fact may come under the sixth class, and may eventually prove to be a novel and unexplained phenomenon, indicating the existence of new laws and forces, complicating but not otherwise interfering with the effects of laws and forces previously known.

The best instance which I can find of an unresolved exceptional phenomenon, consists in the anomalous vapour-densities of phosphorus, arsenic, mercury, and cadmium. It is one of the most important laws of chemistry, discovered by Gay-Lussac, that equal volumes of gases exactly correspond to equivalent weights of the substances, and this holds generally true of any elements which we can convert into gas or vapour. Unfortunately phosphorus and arsenic give vapours exactly twice as dense as they should do by analogy, and mercury and cadmium diverge in the other direction, giving vapours half as dense as we should expect. We cannot treat these anomalies as limiting exceptions, and say that the law holds true of substances generally but not of these; for the properties of gases, as previously noticed (vol. ii. p. 250), usually admit of the surest and widest generalizations. Besides, the preciseness of the ratio of divergence points to the real observance of the law in a modified manner. We might endeavour to reduce the exceptions by doubling the atomic weights of phosphorus and arsenic, and halving those of mercury and cadmium. But this step has of course been maturely considered by chemists, and is found to conflict with all the other analogies of the substances and the principles of isomorphism. One of the most probable explanations is that phosphorus and arsenic produce vapour in an allotropic condition, which might perhaps by intense heat be resolved into a simple gas of half the density; but facts are wholly wanting to support this hypothesis, and it cannot be applied to the other two exceptions without supposing that gases and vapours generally are capable of resolution into something simpler. In short, chemists can at present make nothing of these anomalies. As Hofmann distinctly says, 'Their philosophical interpretation belongs to the future . . . They may turn out to be typical facts, round which many others of the like kind may come hereafter to be grouped; and they may prove to be allied with special properties, or dependent on particular conditions as yet unsuspected[8].'

The expansion of solids and liquids by heat is also a general law, in which we cannot expect to find any real anomalies, any facts indicating too wide generalization, or even any accidental disturbing causes. The contraction of water and several other liquids, even of fusible metal, by heat, together with the few cases in which a solid contracts by heat, must therefore be probably regarded as results of the very law of expansion acting in a complicated and disguised manner. It would be easy to point out an almost infinite number of other unexplained anomalies. Physicists assert, as an absolutely universal law, that in liquefaction heat is absorbed[1], yet sulphur is at least an apparent exception.

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[8] Hofmann's, 'Introduction to Chemistry,' p. 198.

The two substances, Sulphur and Selenium, are remarkable for their relations to heat. Sulphur may almost be said to have two melting points, for, though liquid like water at 120° C., it becomes quite thick and tenacious between 221° and 249°, melting once again at higher temperatures. As well as the other element named, it may be thrown into several curious states, which chemists conveniently dispose of by calling them *allotropic*, a term freely used when they are puzzled to know what has happened. The chemical and physical history of iron, again, is full of anomalies; not only does it undergo inexplicable changes of hardness and texture in its alloys with carbon and other substances, but it is almost the only substance which conveys sound with greater velocity at a higher than at a lower temperature, the velocity increasing from 20° to 100° C., and then decreasing. Silver is also anomalous in regard to sound. These are all instances of inexplicable exceptions, the bearing of which must be ascertained in the future progress of science.

When the discovery of new and peculiar phenomena conflicting with our theories of the constitution of nature is reported to us, it becomes no easy task to steer a philosophically correct course between credulity and scepticism. We are not to assume, on the one hand, that there is any limit to the wonders which nature can present to us. Nothing except the contradictory is really impossible, and many things which we now regard as common-place were considered as little short of the miraculous when first perceived. The electric telegraph was a visionary dream among mediæval physicists; it has hardly yet ceased to excite our wonder; to our descendants centuries hence it will probably appear inferior in ingenuity to some inventions which they will possess. Now every strange phenomenon may be a secret spring which, if rightly touched, will open the door to new chambers in the palace of nature. To refuse to believe, then, in the occurrence of anything new and strange would be to neglect the most precious chances of discovery. We may say with Hooke that 'the believing strange things possible may perhaps be an occasion of taking notice of such things as another would pass by without regard as useless.' We are not, therefore, to shut our ears even to such apparently absurd stories as those concerning second sight, clairvoyance, animal magnetism, ode force, table-turning, or any of the popular delusions which from time to time are current. The facts recorded concerning these matters are facts in some sense or other, and they demand explanation, either as new natural phenomena, or as the results of combined credulity and imposture. Most of the statements concerning the supposed phenomena referred to have been, or by careful investigation would doubtless be, referred to the latter head, and the absence of any appearance of scientific ability or care in many of those who describe them, is sufficient to cast a doubt upon their value. It is mainly upon this ground, and not on account merely of the strangeness and intrinsic improbability of the statements made that we should hesitate to accept them. Certainly in the obscure phenomena of mind, those relating to memory, dreams, somnambulism, and other peculiar actions or states of the nervous system, there are many inexplicable and almost incredible facts, and it is equally unphilosophical to believe or to disbelieve without clear evidence. There are many facts, too, concerning the instincts of animals, and the mode in which they find their way from place to place, which are at present quite inexplicable. We may always feel sure that there are many things not yet dreamt of in our philosophy.

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[1] Stewart's 'Elementary Treatise on

CHAPTER XXX.

CLASSIFICATION.

THE extensive subject of Classification has been deferred to a late part of this treatise, because it involves many questions of difficulty, and did not seem naturally to fall into any earlier place. But it must not be supposed that, in now formally taking up the subject, we are for the first time entertaining the notion of classification. All logical inference involves classification, which is indeed the necessary accompaniment of the action of judgment. It is impossible to detect a point of similarity between two or more objects without thereby joining them together in thought, and thus forming an incipient or potential class. Nor can we ever bestow a common name upon two or more objects without thereby equally implying the existence of a class. Every common name is the name of a class, and every name of a class is a common name. It is evident also that every general notion, or concept is but another way of speaking of a class. Usage alone leads us to use the word classification in some cases and not in others. We are said to form the general notion *parallelogram* when we regard an infinite number of possible four-sided rectilinear figures as resembling each other in the common property of possessing parallel sides. We should be said to form a class, Trilobite, when we place alongside of each other in a museum a number of hand specimens resembling each other in certain defined qualities. But the logical nature of the operation is, or should be, exactly the same in both cases. We form a class of figures called parallelograms, and we form a general notion of Trilobites.

Science, it has been said at the outset, is the detection of identity, and classification is the placing together, either in thought or in actual proximity of space, those notions or objects between which identity has been detected. Accordingly the value of classification is co-extensive with the value of science and general reasoning. Whenever we form a class we reduce multiplicity to unity, and detect, as Plato said, the one in the many. The result of such classification is to yield generalized knowledge, as distinguished from the direct and sensuous knowledge of particular facts. Of every class, so far as it is correctly formed, the great principle of substitution is true, and whatever we know of one object in a class we also know of the other objects, so far as identity has been detected between them. The facilitation and abbreviation of mental labour is at the bottom of all mental progress. The reasoning faculties of Newton were not different in qualitative character from those of a ploughman; the difference lay in the extent to which they were exerted, and the number of facts which could be treated. Every thinking being generalizes more or less, but it is the depth and extent of his generalizations which distinguish the philosopher. Now it is the exertion of the classifying and generalizing powers which thus enables the intellect of man to cope in some degree with the infinite number and variety of natural phenomena and objects. In the chapters upon Combinations and Permutations it was rendered quite evident, that from a few elementary differences immense numbers of various combinations can be produced. The process of classificati [illegible] resolve these combinations, and r [illegible] e according to metals, and have been selected by the ballot. But when chemists carefully selected from the list the five metals, Potassium, Sodium, Cæsium, Rubidium, and Lithium, and called them the Alkaline metals, a great deal was implied in this classification. On comparing the qualities of these substances, they are all found to combine very energetically with oxygen, to decompose water at all temperatures, and to form strongly basic oxides, which are very soluble in water, yielding powerfully caustic and alkaline hydrates from which water cannot be expelled by heat. Their carbonates are also soluble in water, and each metal forms only one chloride. It may also be expected as a general rule that each salt into which one of the five metals enters will correspond to salts into which the other metals enter, there being a general analogy between the properties and compounds of these metals.

Now in forming this class of alkaline metals, we have done more than merely select a convenient order of statement. We have arrived at a discovery of certain empirical laws of nature, the probability being very considerable that a metal which exhibits some of these properties will also possess the others. If we discovered another metal whose carbonate was soluble in water, and which energetically combined with water at all temperatures, producing a strongly basic oxide, we should infer that it would form only a single chloride, and that, generally speaking, it would enter into a series of compounds corresponding to the salts of the other alkaline metals. The formation of this class of alkaline metals, then, is no mere matter of convenience; it is an important and highly successful act of inductive discovery, enabling us to register many undoubted propositions as results of n [illegible] and to make an almost indefinite [illegible] ling upon the principles of in is to be selected, to the exclusion of all others. This erroneous notion probably proceeds also in part from the limited powers of thought and the inconvenient mechanical conditions under which we labour. If we arrange the books in a library catalogue, we must arrange them in some one order; if we compose a treatise on mineralogy, the minerals must be successively described in some one arrangement; if we describe even such simple things as geometrical figures, they must be taken in some fixed order. We shall naturally therefore select that classification which appears to be most convenient and instructive for our principal purpose. But it does not follow that this system of classification possesses any exclusive excellence, and there will be usually many other possible arrangements, each valuable in its own way. A perfect intellect would not confine itself to one order of thought, but would simultaneously regard a group of objects as classified in all the ways of which they are capable. Thus the elements may be classified according to their atomicity into the groups of Monads, Dyads, Triads, Tetrads, Pentads, and Hexads, and this is probably the most instructive classification; but it does not prevent us from also classifying them according as they are metallic or non-metallic, solid, liquid or gaseous at ordinary temperatures, useful or useless, abundant or scarce, ferro-magnetic or diamagnetic, and so on.

Mineralogists have spent a great deal of labour in trying to discover a so-called natural system of classification for minerals. They have constantly encountered the difficulty that the chemical composition did not run together with the crystallographic form, and the various physical properties of the mineral. Substances identical in the form of their cr [illegible] specially those belonging to the first or se [illegible] ls, were often found to have no [illegible] po-

Natural and Artificial Systems of Classification.

It has been usual to distinguish systems of classification as natural and artificial, those being called natural which seemed to express the order of existing things as determined by nature. Artificial methods of classification, on the other hand, included those formed for the mere convenience of men in remembering or treating natural objects.

The difference, as it is commonly regarded, has been well described by Ampère[c], as follows: 'We can distinguish two kinds of classifications, the natural and the artificial. In the latter kind, some characters, arbitrarily chosen, serve to determine the place of each object; we abstract all other characters, and the objects are thus found to be brought near to or to be separated from each other, often in the most bizarre manner. In natural systems of classification, on the contrary, we employ concurrently all the characters essential to the objects with which we are occupied, discussing the importance of each of them; and the results of this labour are not adopted unless the objects which present the closest analogy are brought most near together, and the groups of the several orders which are formed from them are also approximated in proportion as they offer more similar characters. In this way it arises that there is always a kind of connexion, more or less marked, between each group and the group which follows it.'

There is much, however, that is vague and logically false in this and many other definitions which have been proposed by naturalists to express their notion of a natural system. We are not informed how the *import-*

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[c] 'Essai sur l

ance of a resemblance is to be determined, nor what is the measure of the *closeness* of analogy. Until all the words employed in a definition are made clear in meaning, the definition itself is worse than useless. Now if the views concerning classification here upheld are true, there can be no sharp and precise distinction between natural and artificial systems. All arrangements which serve any purpose at all must be more or less natural, because, if closely enough scrutinized, they will involve more resemblances than those whereby the class was defined.

It is true that in the biological sciences there would be one arrangement of plants or animals which would be conspicuously instructive, and in a certain sense natural, if it could be attained, and it is that after which naturalists have been in reality striving for nearly two centuries, namely, that arrangement which would display the genealogical descent of every form from the original life germ. Those morphological resemblances upon which the classification of living beings is almost always based are inherited resemblances, and it is evident that descendants will usually resemble their parents and each other in a great many points.

I have said that a natural is distinguished from an arbitrary or artificial system only in degree. It will be found almost impossible to arrange objects according to any one circumstance without finding that some correlation of other circumstances is thus made apparent. No arrangement could seem more arbitrary than the common alphabetical arrangement according to the initial letters of the name. But we cannot scrutinize a list of names of persons without noticing a predominance of Evans's and Jones's, under the letters E and J, and of names beginning with Mac under the letter M. The predominance is so great that we could not attribute it to chance, and inquiry would of course show that it arose from important facts concerning the nationality of the persons. It would appear that the Evans's and Jones's were of Welsh descent, and those whose names bear the prefix Mac of Scotch descent. With the nationality would be more or less strictly correlated many peculiarities of physical constitution, language, habits, or mental character. In other cases I have been interested in noticing the empirical inferences which are displayed in the most apparently arbitrary arrangements. If a large register of the names of ships be examined it will often be found that a number of ships bearing the same name were built about the same time, a correlation due to the occurrence of some striking incident shortly previous to the building of the ships. The age of ships or other structures is usually closely correlated with their general form, nature of materials, &c.

It is impossible to examine the details of some of the most apparently artificial systems of classification of plants, without finding that many of the classes are natural in character. Thus in Tournefort's arrangement, depending almost entirely on the formation of the corolla, we find the natural orders of the Labiatæ, Cruciferæ, Rosaceæ, Umbelliferæ, Liliaceæ, and Papilionaceæ, recognised in his 4th, 5th, 6th, 7th, 9th, and 10th classes. Many of the classes in Linnæus' celebrated sexual system also approximate to natural classes.

Correlation of Properties.

Habits and usages of language are always apt to lead us into the error of imagining that when we employ different words we mean different things. In introducing the subject of classification nominally I was careful to draw the reader's attention to the fact that all reasoning and all operations of scientific method really involve classification, though we are accust [illegible] me in some cases and not in others. Now the name *correlation* requires to be used with the same qualification. Things are correlated (*con, relata*) when they are so related or bound to each other that where one is the other is, and where one is not the other is not. Throughout this work we have then been dealing with correlations. In geometry the occurrence of three equal angles in a triangle is correlated with the existence of three equal sides; in physics gravity is correlated with inertia; in botany exogenous growth is correlated with the possession of two cotyledons, or the production of flowers with that of spiral vessels. But it is in the classificatory sciences especially that the word correlation has been employed.

We find it stated that in the class Mammalia the possession of two occipital condyles, with a well-ossified basi-occipital, is correlated with the possession of mandibles, each ramus of which is composed of a single piece of bone, articulated with the squamosal element of the skull, and also with the possession of mammæ and non-nucleated red blood-corpuscles. Professor Huxley remarks[d] that this statement of the character of the class mammalia is something more than an arbitrary definition; it is a statement of a law of correlation or co-existence of animal structures, from which most important conclusions are deducible. It involves a generalization to the effect that in nature the structures mentioned are always found associated together. This simply amounts to saying that the formation of the class mammalia involves an act of inductive discovery, and results in the establishment of certain empirical laws of nature. Professor Huxley has excellently expressed the mode in which discoveries of this kind enable naturalists to make deductions or predictions with considerable confidence, but he has also pointed out that such inferences are likely from time to time to prove mistaken. I will quote his own words:

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[d] 'Lectures on the Elements of Comparative Anatomy, and on the Classification of Animals,' 1864, p. 3.

'If a fragmentary fossil be discovered, consisting of no more than a ramus of a mandible, and that part of the skull with which it articulated, a knowledge of this law may enable the palæontologist to affirm, with great confidence, that the animal of which it formed a part suckled its young, and had non-nucleated red blood-corpuscles; and to predict that should the back part of that skull be discovered, it will exhibit two occipital condyles and a well-ossified basi-occipital bone.

'Deductions of this kind, such as that made by Cuvier in the famous case of the fossil opossum of Montmartre, have often been verified, and are well calculated to impress the vulgar imagination; so that they have taken rank as the triumphs of the anatomist. But it should carefully be borne in mind, that, like all merely empirical laws, which rest upon a comparatively narrow observational basis, the reasoning from them may at any time break down. If Cuvier, for example, had had to do with a fossil Thylacinus instead of a fossil Opossum, he would not have found the marsupial bones, though the inflected angle of the jaw would have been obvious enough. And so, though, practically, any one who met with a characteristically mammalian jaw would be justified in expecting to find the characteristically mammalian occiput associated with it; yet, he would be a bold man who should strictly assert the belief which is implied in this expectation, viz., that at no period of the earth's history did animals exist which combined a mammalian occiput with a reptilian jaw, or vice versa.'

One of the most dist[illegible] correlation in the anim[illegible] *ruminating animals*, and [illegible] than in the following extract from the classical work of Cuvier[e]:

'I doubt if any one would have divined, if untaught by observation, that all ruminants have the foot cleft, and that they alone have it. I doubt if any one would have divined that there are frontal horns only in this class: that those among them which have sharp canines for the most part lack horns.

'However, since these relations are constant, they must have some sufficient cause; but since we are ignorant of it, we must make good the defect of the theory by means of observation: it enables us to establish empirical laws which become almost as certain as rational laws when they rest on sufficiently repeated observations; so that now whoso sees merely the print of a cleft foot may conclude that the animal which left this impression ruminated, and this conclusion is as certain as any other in physics or morals. This footprint alone, then, yields to him who observes it, the form of the teeth, the form of the jaws, the form of the vertebræ, the form of all the bones of the legs, of the thighs, of the shoulders, and of the pelvis of the animal which has passed by: it is a surer mark than all those of Zadig.'

We meet with a good instance of the purely empirical correlation of circumstances when we classify the planets of the solar system according to their densities or periods of axial rotation[f]. If we examine a table specifying the usual astronomical numbers of the solar system, we find that four planets resemble each other very closely in the period of axial rotation, and the same four planets are all found to have high densities, thus:—

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[e] 'Ossemens Fossiles,' 4th edit. vol. i. p. 164. Quoted by Huxley, 'Lectures,' &c., p. 5.

[f] Chambers, 'Descriptive Astronomy,' 1st edit. p. 23.

Name of Planet.	Period of Axial Rotation.	Density.
Mercury	24 hours 5 minutes	7·94
Venus	23 „ 21 „	5·33
Earth	23 „ 56 „	5·67
Mars	24 „ 37 „	5·84

Forming a similar table for the other chief planets, it is as follows :—

Name of Planet.	Period of Axial Rotation.	Density.
Jupiter	9 hours 55 minutes	1·36
Saturn	10 „ 29 „	·74
Uranus	9 „ 30 „	·97
Neptune	— —	1·02

It will of course be observed that in neither group is the equality of the rotational period or of the density more than rudely approximate, nevertheless the difference of the numbers in the first and second group is so very marked, the periods of the first being at least double and the densities four or five times those of the second, that the coincidence cannot be attributed to accident. The reader will also notice that the first group consists of the planets nearest to the sun, that with the exception of the earth none of them possess satellites, and that they are all comparatively small; the second group are furthest from the sun, and all of them possess several satellites, and are comparatively great. Therefore, with but slight exception, the following correlations hold true :—

Interior planets.	Long period.	Small size.	High density.	No satellites.
Exterior „	Short „	Great „	Low „	Many „

These coincidences certainly point with much probability to a difference in the conditions of origin of the two groups, but no further explanation of the matter is yet possible.

The classification of comets by Mr. Hind and Mr. A. S. Davis according to their periods tends to establish the conclusion that distinct groups of comets have been brought into the solar system by the attractive powers of Jupiter, Uranus, or other planets[g]. The classification of nebulæ as commenced by the two Herschels, and continued by Lord Rosse, Mr. Huggins, and others, will probably lead at some future time to the discovery of important empirical laws concerning the constitution of the universe. The minute examination and classification of meteorites, as carried on by Mr. Sorby and others, seems likely to afford us an insight into the constitution of the material universe.

We should never fail to remember and record the slightest and most apparently inexplicable coincidences or correlations, for they may prove of importance in the future. Discoveries begin when we are least expecting them. It is a very significant fact that the greater number of variable stars are of a reddish colour. Not all variable stars are red, nor all red stars variable, but considering that only a small fraction of the observed stars are known to be variable, and only a small fraction are red, the number which fall into both classes is too great to be accidental[h]. It is also remarkable that the greater number of stars possessing great proper motion are double stars, the star 61 Cygni being especially noticeable in this respect[i]. The correlation in these cases is not perfect and without exception, but the preponderance is so great as to point to some natural correlation, the exact nature of which must be a matter for future investigation. Sir John Herschel has remarked that the two double stars 61 Cygni and α Centauri of which the orbits were well ascertained, evidently belonged to the same family or genus[k].

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[g] 'Philosophical Magazine,' 4th Series, vol. xxxix. p. 396; vol. xl. p. 183; vol. xli. p. 44.

[h] Humboldt, 'Cosmos,' (Bohn) vol. iii. p. 224.

[i] Baily, 'British Association Catalogue,' p. 48.

[k] 'Outlines of Astronomy,' § 850, 4th edit. p. 578.

Classification in Crystallography.

One of the most perfect and instructive instances of classification which we can find is furnished by the science of crystallography, already briefly noticed (vol. i. p. 153). The system of arrangement now generally adopted is conspicuously natural, and is even mathematically perfect. A crystal consists in every part of similar molecules similarly related to the adjoining molecules, and connected with them by forces the nature of which we can only learn by their apparent effects. But these forces are exerted in space of three dimensions, so that there is a limited number of suppositions which can be entertained as to the relations of these forces. In one case each molecule will be similarly related to all those which are next to it; in a second case, it will be similarly related to those in a certain plane, but differently related to those not in that plane. In the simpler cases the arrangement of molecules is rectangular; in the remaining cases oblique either in one or two planes.

In order to simplify the explanation and conception of the complicated phenomena which crystals exhibit, an hypothesis has been invented which is an excellent illustration of the class of Descriptive Hypotheses before mentioned (vol. ii. p. 153). Crystallographers imagine that there are within each crystal certain axes, or lines of direction, by the comparative length and the mutual inclination of which the nature of the crystal is determined and recorded. In one somewhat exceptional class of crystals there are three such axes lying in one plane, and a fourth perpendicular to that plane; but in all the other classes there are imagined to be only three axes. Now these axes can be varied in three ways as regards length: (1) they may be all equal, or (2) two equal and one unequal, or (3) all unequal. They may also be varied in four ways as regards direction: (1) they may be all at right angles to each other; (2) two axes may be at right angles and the third perpendicular to one of them and oblique to the other; (3) two axes may be at right angles to each other and the third oblique to both; (4) the three axes may be all oblique to each other. Now if all the variations as regards length were combined with those regarding direction, it would seem to be possible to have twelve classes of crystals in all, the enumeration being then logically and geometrically complete. But as a matter of empirical observation, many of these classes are not found to occur, oblique axes being seldom or never equal. There remain in all seven distinct classes of crystals, but even of these one class is not positively known to be represented in nature.

The first class of crystals is defined by possessing three equal rectangular axes, and equal elasticity in all directions. The primary or most simple form of the crystals is the cube, but by the modification or removal of the corners of the cube by planes variously inclined to the axes, we have the regular octohedron, the dodecahedron, or various combinations of these forms. Now it is a law of this class of crystals that as each axis is exactly like each of the other two, every modification of any corner of a crystal must be repeated symmetrically with regard to the other axes; thus the forms produced are symmetrical or regular, and the class is called the *Regular System* of Crystals. It includes a great variety of substances, some of them being elements, such as carbon in the form of diamond, others more or less complex compounds, such as rock-salt, potassium iodide and bromide, the several kinds of alum, fluor-spar, iron bisulphide, garnet, spinelle, &c. No correlation then is apparent between crystallization and the chemical composition. But what we have to notice is that the physical properties of the crystallized substances with regard to light, heat, electricity, &c., are closely similar. Light and heat undulations, wherever they enter a crystal of the regular system, spread with equal rapidity in all directions, just as they would in a uniform liquid, gas, or amorphous solid, such as unstrained glass. Crystals of the regular system accordingly do not in any case exhibit the phenomena of double refraction, unless by mechanical compression we alter the conditions of elasticity. These crystals, again, expand equally in all directions when heated, and if we could cut a sufficiently large plate from a cubical crystal, and examine the sound vibrations of which it is capable, we should find that they indicated an equal elasticity in every direction. Thus we see that a great number of important properties are correlated with that of crystallizing in the regular system, and as soon as we know that the primary form of crystallization of a substance is the cube, we are able to infer with approximate certainty that it possesses all these properties. The class of cubical crystals is then an evidently natural class, one disclosing general laws connecting together the physical and mechanical properties of the substances so classified.

In the second class of crystals, called the dimetric, square prismatic, or pyramidal system, there are also three axes at right angles to each other, two of which are equal, and the third or principal axis is unequal, being either greater or less than either of the other two. In such crystals accordingly the elasticity and other physical properties are alike in all directions perpendicular to the principal axis, but vary in all other directions. If a point within a crystal of this system be heated, the heat spreads with [illegible] planes perpendicular to the principal axis, but more or less rapidly in the direction of this axis, so that the isothermal surface is an ellipsoid of revolution round that axis.

Nearly the same statement may be made concerning the third or hexagonal or rhombohedral system of crystals, in which there are three axes lying in one plane and meeting at angles of 60°, while the fourth axis is perpendicular to the other three. The hexagonal prism and the rhombohedron are the two commonest forms assumed by crystals of this system, and in ice, quartz, and calc-spar, we have abundance of beautiful specimens of the various forms produced by the modification of the primitive form. Calc-spar alone is said to crystallize in at least 700 varieties of forms. Now of all the crystals belonging both to this and the dimetric class, we know that a ray of light passing in the direction of the principal axis will be refracted singly as in a crystal of the regular system; but in every other direction the light will suffer double refraction being separated into two rays, one of which obeys the ordinary law of refraction, but the other a much more complicated law. The other physical properties vary in an analogous manner. Thus calc-spar expands by heat in the direction of the principal axis, but contracts by a small quantity in directions perpendicular to it. So closely indeed are these various physical properties correlated that Mitscherlich, having observed the law of expansion in calc-spar, was enabled to predict that the double refracting power of the substance would be decreased by a rise of temperature, as was proved by experiment to be the case.

In the fourth system, called the trimetric, rhombic, or right prismatic system, there are three axes, at right angles, but all unequal in length. It may be asserted in general terms that the mechanical properties vary in such crystals in every direction, and heat spreads so that the isothermal surface is an ellipsoid with three unequal axes.

In the remaining three classes, called the monoclinic, diclinic, and triclinic, the axes are more or less oblique, as described above (vol. ii. p. 360), and at the same time unequal. The complication of phenomena is therefore greatly increased, and it need only be stated that there are always two directions in which a ray is singly refracted, but that in all other directions double refraction takes place. The conduction of heat is unequal in all directions, the isothermal surface being an ellipsoid of three unequal axes. The relations of such crystals to other phenomena are often very complicated, and hardly yet reduced to law. Thus some crystals, called pyro-electric, manifest vitreous electricity at some points of their surface, and resinous electricity at other points when rising in temperature, the character of the electricity being changed when the temperature sinks again. This production of electricity is believed indeed to be connected with the hemihedral character of the crystals exhibiting it. The crystalline structure of a substance again influences its magnetic behaviour, the general law being that the direction in which the molecules of a crystal are most closely approximated tends to place itself axially or equatorially between the poles of a magnet, according as the body is magnetic or diamagnetic. Further questions arise if we apply pressure to crystals. Thus doubly refracting crystals with one principal axis acquire two axes when the pressure is perpendicular in direction to the principal axis.

All the phenomena peculiar to crystalline bodies are thus closely correlated with the formation of the crystal, or will almost certainly be found to be so as investigation proceeds. It is upon ex [illegible] ation indeed that the laws of connexio [illegible] founded, but in the classes varying from three to twenty elements. Now if we were to calculate the whole number of ways in which sixty-three objects can be arranged in six groups, we should find the number to be so great that the life of the longest lived man would be wholly inadequate to enable him to go through these possible groupings. The rule of exhaustive arrangement, then, is absolutely impracticable. It follows also that mere haphazard trial cannot as a general rule give any useful result. If we were to write the names of the elements in succession upon sixty-three cards, throw them into a ballot-box, and draw them out haphazard in six handfuls time after time, the probability is excessively small that we take them out at any one trial in a specified order, for instance that at present adopted by chemists.

The usual mode in which an investigator proceeds to form a classification of any new group of objects, seems to consist in tentatively arranging them according to their most obvious similarities. Any two objects which present a close resemblance to each other will be joined and formed into the rudiment of a class, the definition of which will at first include all the apparent points of resemblance. Other objects as they come to our notice will be gradually assigned to those groups with which they present the greatest number of points of resemblance, and the definition of a class will often have to be altered in order to admit them. The early chemists, for instance, could hardly avoid classing together the common metals, gold, silver, copper, lead, and iron, which present such conspicuous points of similarity as regards density, metallic lustre, malleability, &c. With the progress of discovery, however, difficulties begin to present themselves in such a grouping. Antimony, bismuth, and arsenic are distinctly metallic as regards lustre, density, and some chemical properties, but are wanting in mallefessor G. C. Foster in Watts's 'Dictionary of Chemistry.' But the present theory of classification was not reached until at least three previous false systems had been long entertained. And though there is much reason to believe that the present system of classification according to atomicity is substantially correct, many errors may yet be discovered in the details of the grouping.

Symbolic Statement of the Theory of Classification.

The whole theory of classification can be explained in the most complete and general manner, by reverting for a time to the use of the Logical Abecedarium, which was found to be of supreme importance in Formal Logic (vol. i. p. 109). That form expresses in fact the necessary classification of all objects and ideas as depending on the laws of thought, and there is no point concerning the purpose and methods of classification which may not be explained most precisely by the use of letter combinations, the only inconvenience being the somewhat abstract and repulsive form in which the subject is thus represented.

If we pay regard only to three qualities or circumstances in which things may resemble each other, namely the qualities A, B, C, then there are according to the laws of thought eight possible classes of objects. If there exist objects belonging to all these eight classes, thus indicated,

ABC
AB*c*
A*b*C
A*bc*
*a*BC
*a*B*c*
*ab*C
abc

it follows that the qualities A, B, C are subject to no conditions except the primary laws of thought and nature (vol. i. p. 6). There is then no special law of nature to

ABC*d*E	*a*BC*d*E
AB*cde*	*a*B*cde*
A*b*CDE	*ab*CDE
A*bc*D*e*	*abc*D*e*.

They are now classified, so that those containing A stand first, and those devoid of A second, but no other property seems to be correlated with A. Let us alter this arrangement and group the combinations as follows:—

ABC*d*E	A*b*CDE
AB*cde*	A*bc*D*e*
*a*BC*d*E	*ab*CDE
*a*B*cde*	*abc*D*e*.

It requires very little examination to discover that, in the first group, B is always present and D absent, whereas in the second group, B is always absent and D present. This is the result which follows from a law of the form $B = d$ (see vol. i. p. 157), so that in this mode of arrangement we readily discover a close correlation between two letters. Altering the groups again as follows:—

ABC*d*E	AB*cde*
*a*BC*d*E	*a*B*cde*
A*b*CDE	A*bc*D*e*
*ab*CDE	*abc*D*e*,

we discover another evident correlation between C and E. Between A and the other letters, or between the two pairs of letters B, D and C, E there is no logical connexion whatever.

This example may perhaps seem tedious, but it will be found instructive in this way. We are classifying only seven objects or combinations, in each of which only five qualities are considered. There are only two laws of correlation between four of those five qualities, and those laws are of the simplest logical character. Yet the reader would hard[illegible] what those laws were, and confidently assign them by mere contemplation of the combinations, as given in the first group. Several tentative classifications must probably be made before we can resolve the question. Let us now suppose that instead of seven objects and five qualities, we have, say, five hundred objects and fifty qualities. If we were to attempt the same method of exhaustive grouping which we before employed, we should have to arrange the five hundred objects in fifty different ways, before we could be sure that we had discovered even the simpler laws of correlation. But even the successive grouping of all those possessing each of the fifty properties would not necessarily give us all the laws. There might exist complicated relations between several properties simultaneously, for the detection of which no rule of procedure whatever can be given.

If the reader entertains any doubt as to the difficulty of classifying combinations so as to disclose their relations, let him test the matter practically upon the following series of combinations. They involve only six letters denoting six qualities, which are subject to four laws of correlation of no great complexity.

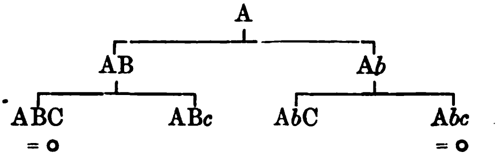
ABCDEF
ABCD*e*F
ABCD*ef*
ABC*d*E*f*
AB*c*DEF
AB*c*D*e*F
AB*c*D*ef*
A*bcd*E*f*
*a*B*c*DEF
*a*B*c*D*e*F
*a*B*c*D*ef*
*abcd*E*f*.

I shall be happy to receive the solution of the above problem in classification from any reader who thinks he has solved it; that is to say, I shall be glad to ascertain whether any reader succeeds in detecting the laws of correlation between the letters, which yield the above combinations, according to the principles of the Indirect Method described in Chapter VI.

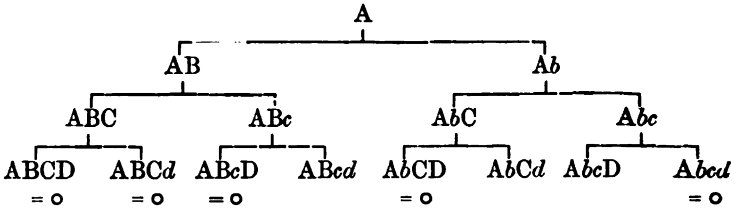
Bifurcate Classification.

Every system of classification ought theoretically to be formed on the principles of the Logical Abecedarium. Each superior class should be divided into two inferior classes, distinguished by the possession and non-possession of a single specified property. Each of these minor classes, again, is divisible by any other property whatever which can be suggested, and thus every classification logically consists of an infinitely extended series of subaltern genera and species. The classifications which we form are in reality very small fragments of those which would correctly and fully represent the relations of existing things. But if we take more than four or five qualities into account, the number of subdivisions grows impracticably large. Our finite minds are unable to treat any complex group exhaustively, and we are obliged to simplify and generalize scientific problems, often at the risk of overlooking particular conditions and exceptions.

Every system of classes displayed in the manner of the Logical Abecedarium may be called *bifurcate*, because every class branches out at each step into two minor classes, existent or imaginary. It would be a great mistake to regard this arrangement as in any way a peculiar or special method; it is not only a natural and important one, but it is the inevitable and only system which is logically perfect, according to the fundamental laws of thought. All other arrangements of classes correspond to the bifurcate arrangement, with the implication that some of the minor classes are not represented among existing things. If we take the genus A and divide it into the species AB and AC, we imply two propositions, namely that in the class A, the properties of B and C never occur together, and that they are never both absent; these propositions are indeed logically equivalent to one, namely $AB = Ac$. Our classification is then identical with the following bifurcate one:—



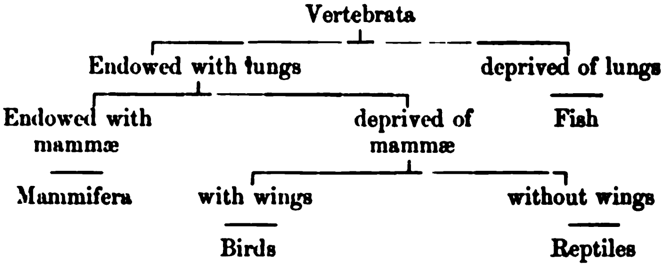
If, again, we divide the genus A into three species AB, AC, AD, we are either logically in error, or else we must be understood to imply the existence of three propositions excluding the union within the genus A of the properties of B, C and D, namely $AB = ABcd$, $AC = AbCd$, and $AD = AbcD$. It comes to the same thing if we say that our classification is really a bifurcate one, as follows:—



The logical necessity of bifurcate classification has been clearly and correctly stated in the 'Outline of a New System of Logic' by George Bentham, a work of which the logical value has been quite overlooked until lately. Mr. Bentham points out, in p. 113, that every classification must be essentially bifurcate and takes, as an example, the division of vertebrate animals into four subclasses, as follows:—

Mammifera—	endowed with mammæ and lungs.
Birds	without mammæ but with lungs and wings.
Fish	deprived of lungs.
Reptiles	deprived of mammæ and wings but with lungs.

We have, then, as Mr. Bentham says, three bifid divisions, thus represented:--



It is however quite evident that according to the laws of thought even this arrangement is incomplete. The subclass mammifera must either have wings or be deprived of them; we must subdivide this class, or assume that none of the mammifera have wings, which is, as a matter of fact, the case, the wings of bats not being true wings in the meaning of the term as applied to birds. Fish, again, ought to be considered with regard to the possession of mammæ and wings; and in leaving them undivided we really imply that they never have mammæ nor wings, the wings of the flying-fish, again, being no exception. If we resort to the use of our letters and define them as follows—

A = vertebrata,
B = having lungs,
C = having mammæ,
D = having wings,

then there are four existent classes of vertebrata which appear to be thus described—

ABC
ABcD

But in realit o be

exhaustive classification in respect to the complexity of the ratios forming them. Plane rectilinear figures may also be classified according to the number of their sides as triangles, quadrilateral figures, pentagons, hexagons, heptagons, &c. The bifurcate arrangement is not false when applied to such series of objects; it is even necessarily involved in the arrangement which we do apply, so that its formal statement is needless and tedious. The same may be said of the division of portions of space. Reid and Kames endeavoured to cast ridicule on the bifurcate arrangement[n] by proposing to classify the parts of England into Middlesex and what is not Middlesex, dividing the latter again into Kent and what is not Kent, the latter again into Sussex and what is not Sussex; and so on. This is so far, however, from being an absurd proceeding that it is requisite to assure us that we have made an exhaustive enumeration of the parts of England.

The Five Predicables.

As a general rule it is highly desirable to consign to oblivion all the ancient logical names and expressions, which have infested the science for many centuries past. If logic is ever to be a useful and progressive science, logicians must distinguish between logic and the history of logic. As in the case of any other science it may be desirable to examine the course of thought by which logic has, before or since the time of Aristotle, been brought to its present state; the history of a science is always instructive as giving instances of the mode in which discoveries take place. But at the same time we ought [illegible] e statement of the science

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[illegible] New System of Logic,' p. 115.

itself of all names and other vestiges of antiquity which are not actually useful at the present day.

Among those ancient expressions which may well be excepted from such considerations and ever retained in use, are the 'Five Words' or 'Five Predicables' which were described by Porphyry in his 'Introduction to Aristotle's Organum.' Two of them indeed, namely *Genus* and *Species*, are the most venerable names in philosophy, having probably been first employed in their present logical meanings by Socrates. In the present day it requires some mental effort, as Mr. Georges Lewes has remarked[o],' to see anything important in the invention of notions now so familiar as those of Genus and Species. But in reality the introduction of such terms showed the rise of the first germs of logic and scientific method: it showed that men were beginning to analyse their processes of thought.

The Five Predicables are Genus, Species, Difference, Property, and Accident, or in the original Greek *γένος*, *εἶδος*, *διαφορὰ*, *ἴδιον*, *συμβεβηκός*. Of these, Genus may be taken to mean any class of objects which is regarded as broken up into two minor classes, which form Species of it. The Genus is defined by a certain number of qualities or circumstances which belong to all objects included in the class, and which are sufficient to mark out these objects from all others which we do not intend to include. Interpreted as regards intension, then, the Genus is a group of qualities; interpreted as regards extension, it is a group of objects possessing those qualities. If now another quality be taken into account which is possessed by some of the objects and not by the others, this quality becomes a Difference which divides the Genus into two Species. We may interpret the Species either in intension or extension; in the former respect it is more than the Genus as containing one more quality, the Difference: in the latter respect it is less than the Genus as containing only a portion of the group constituting the Genus. We may say then, with Aristotle, that in one sense the Genus is in the Species, namely in intension, and in another sense the Species is in the Genus, namely in extension. The Difference, it is evident, can be interpreted in intension only.

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[o] 'Biographical History of Philosophy,' (1857) vol.
'History of Greece,' vol. viii. p. 578.

A Property is a quality which belongs to the whole of a class, but does not enter into the definition of that class. Thus if it be a generic property it belongs to every individual object contained in the genus. It is a property of the genus Parallelogram that the opposite angles are equal. If we regard a Rectangle as a species of parallelogram, the difference being that *one* angle is a right angle, it follows as a specific property that all the angles are right angles. Though a property in the strict logical sense must belong to each of the objects included in the class of which it is a property, it may or may not belong to other objects. The property of having the opposite angles equal may belong to many figures besides parallelograms, for instance, regular hexagons. It is a property of the circle that all triangles constructed upon the diameter with the apex upon the circumference are right angled triangles, and *vice versâ*, all closed curves of which this is true must be circles. We might with advantage distinguish properties which thus belong to a class, and only to that class, as *peculiar properties.* They enable us to make statements in the form of simple identities (vol. i. p. 44). Thus we know it to be a peculiar property of the circle that for a given length of perimeter it encloses a greater area than any other possible curve; hence we may say—

Curve o[illegible]ture = curve of greatest area.

Here we see that wherever A is C is also found, so that C is a generic property ; D occurs always with B, so that it constitutes a specific property, while E is indifferently present and absent, so as not to be in any way correlated with any of the other letters ; it represents, therefore, an accident. It will now be seen that the Logical Abecedarium really represents an interminable series of subordinate genera and species ; it is but a concise symbolic statement of what was involved in the ancient doctrine of the Predicables.

Summum Genus and Infima Species.

As a genus means any class whatever which is regarded as composed of minor classes or species, it follows that the same class will be a genus in one point of view and a species in another. *Metal* is a genus as regards *alkaline metal*, a species as regards *element*, and any extensive system of classes consists of a series of subordinate, or as they are technically called, *subaltern* genera and species. The question, however, arises, whether any such chain of classes has a definite termination at either end. The doctrine of the old logicians was to the effect that it terminated upwards in a *genus generalissimum* or *summum genus*, which was not a species of any wider class. Some very general notion, such as *substance*, *object* or *thing*, was supposed to be so comprehensive as to include all thinkable objects, and for all practical purposes this might be so. But as I have already explained (vol. i. p. 88), we cannot really think of any object or class without thereby separating it from what is not that object or class. All thinking is relative, and implies discrimination, so that every class and every logical notion must have its negative. If so, t [illegible] g as a *summum*

Bentham indeed long ago stated[p]. We may always put an arbitrary limit to the subdivisions of our classification at any point convenient to our purpose. The crystallographer would not generally consider as different species of crystalline form those which differ only in the degree of development of the faces. The naturalist overlooks innumerable slight differences between plants or animals which he refers to the same species. But in a strictly logical point of view classification might be carried on so long as there is a single point of difference, however minute, between two objects, and we might thus go on until we arrived at individual objects which are numerically distinct in the logical sense attributed to that expression in the chapter upon Number. We must either, then, call the individual the *infima species* or allow that there is no such species at all.

The Tree of Porphyry.

The bifurcate method of classification, arising as it does from the primary laws of thought, is the very foundation of all strict scientific method, and its application in formal logic constitutes the method of Indirect Inference, of which the nature and importance were shown in Chapter VI. So slight, however, has been the attention paid to this all important subject, that I shall in this case break the rule which I have laid down for myself, not to mingle the subject of logic as a science with the history of logic.

Both Plato and Aristotle were fully acquainted with the value of bifurcate division which they occasionally employed in an explicit manner. It [illegible] ssible, too, that Aristotle should state the laws of thought, and employ the predicables without implicitly recognising the logical necessity of that method. It is, however, in Porphyry's remarkable and in many respects excellent 'Introduction to the Categories of Aristotle' that we find the most distinct account of it. Porphyry not only fully and accurately describes the Predicables, but incidently introduces an example for illustrating those predicables, which constitutes a good specimen of bifurcate classification. Translating his words[q] freely we may say that he takes Substance as the genus to be divided, under which are successively placed as Species—Body, Animated Body, Animal, Rational Animal, and Man. Under Man, again, come Socrates, Plato, and other particular men. Now of these notions Substance is the genus generalissimum, and is a genus only, not a species. Man, on the other hand, is the species specialissima (infima species), and is a species only, not a genus. Body is a species of substance, but a genus of animated body, which, again, is a species of body but a genus of animal. Animal is a species of animated body, but a genus of rational animal, which, again, is a species of animal, but a genus of man. Finally, man is a species of rational animal, but is a speci[illegible] and not a genus, being divisible only into [illegible] men.

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[p] 'Outline of a New Syst

Porphyry proceeds at some length to em[illegible] example in further illustration of the predica[illegible] do not find in Porphyry's own [illegible] any [illegible] diagram exhibiting this curious [illegible]men of [illegible]cation, but some of the earlier com[illegible]es and [illegible] writers drew what has long be[illegible] the [illegible] Porphyry.

Thus in the 'Epitome Logica' of Ni[illegible] we find a diagram[r] of which the following is nearly a facsimile :—

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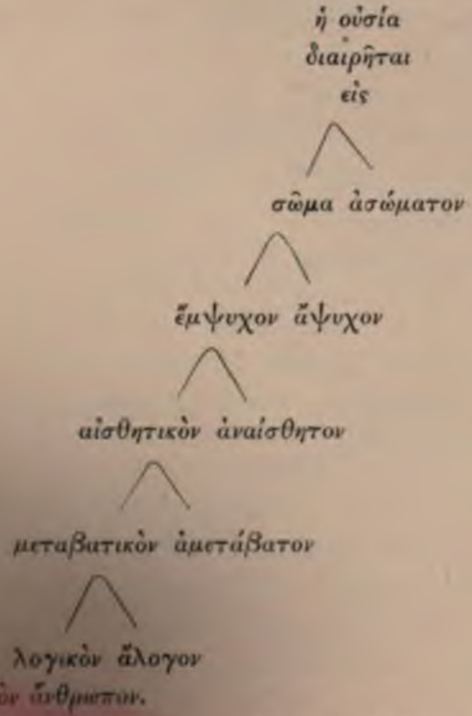
[q] 'Porphyrii Isagoge,' Capu[illegible]



In the above scheme we find the bifurcate principle accurately but incompletely applied. Each genus is subdivided into [illegible] species, described by a pair of positive and negative [illegible] so that the species are together equal in extent [illegible] genus. But it will of course be observed that [illegible] branch is left without further [illegible] only a single infima species, [illegible] ty-two final branches, as there [illegible] complete system.

Th[illegible] reproduced in the works of [illegible] in a form which is more comp[illegible] that of Nicephorus. Thus in the 'Opuscula' of Aquinas, as quoted by Mansel in his edition of Aldrich's 'Artis Logicæ Rudimenta,' second edition, p. 31, we find the Tree nearly in the following form:—

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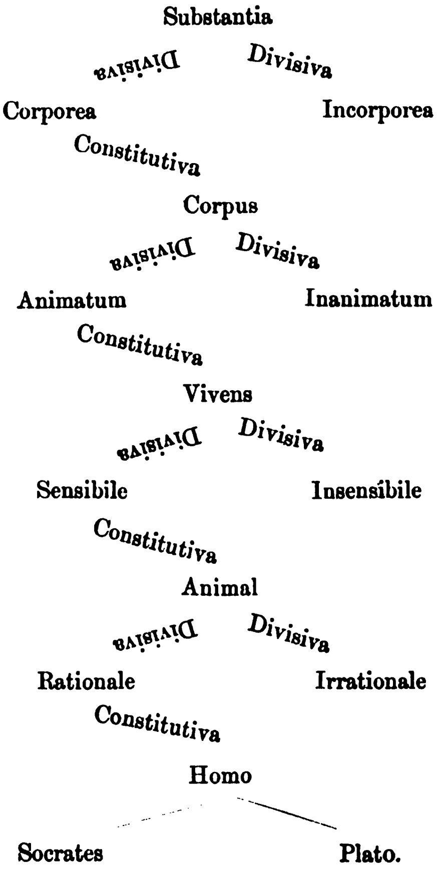
[r] [illegible] Vindel.' 1605, p. 118.



This example of the bifurcate method, although repeated in almost all compendiums and treatises on logic, attracted no particular attention until the time of Peter Ramus and his followers, who are commonly said to have bestowed so much attention and praise upon it as to be regarded by some persons as its inventors. The Ramean Tree is a name frequently employed instead of the Porphyrian Tree, or the κλῖμαξ, that is, the Ladder of Porphyry, as it was sometimes called by the Greek logicians. Although I have looked through several commentaries upon the Dialectics of Ramus, I do not find that very much is said upon the subject. In the Questions of Frederick Beurhusius[r], the method of dichotomy is described as 'illa naturalis et antiquissimorum philosophorum præstantissima Dichotomia,' but in none of the works do I find the Tree itself given.

Among modern logicians Jeremy Bentham possesses the great merit of having drawn attention to the logical importance of bifurcate division. His remarks on the subject are contained in that extraordinary collection of digressive, and often almost incomprehensible papers, called Chrestomathia[s], two of the formidable title-pages of which are given below. The fifth appendix in this work, forming the larger and most important part of the book, consists of an Essay on Nomenclature and Classification[t]. Although written in his later and worse style, this essay is well worth reading, and full of forcible remarks. It may be regarded, I believe, as the first of the series of English writings which have, in the present century, made logic a new and progressive science. In Table IV. Bentham gives the Arbor Porphyriana, as exhibited in the course of a college lecture in 1761, calling it the original form. His reading of logic seems to have been restricted to the compendiums of Saunderson and Watts, and it was only after the text was written that he obtained an opportunity of consulting the work of Porphyry, and was surprised to find no diagram therein. He attributes its invention to Peter Ramus, although he had never seen the writings of that logician, and had merely learnt their titles from a dictionary.

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[r] In Petri Rami, Regii Professoris Clariss. Dialecticæ Libros duos Lutetiæ Anno LXXII, postremo sine Prælectionibus æditos, explicationum Quæstiones: quæ Pædagogiæ Logicæ de Docenda Discendaque Dialectica. Auctore Frederico Beurhusio. Londoni, 1581, p. 120.

[s] 'Chrestomathia: being a Collection of Papers, explanatory of the Design of an Institution proposed to be set on foot, under the name of the Chrestomathic Day School, or Chrestomathic School, for the extension of the New System of Instruction,' &c. By Jeremy Bentham, Esq., London, 1816.

[t] 'An Essay on Nomenclature and Classification: including a Critical Examination of the Encyclopædical Table of Lord Bacon, as improved by D'Alembert: and the first lines of a new one grounded on the application of the Logical Principle of Exhaustively Bifurcate Analysis.' London, 1817.

In this essay he states in the most powerful way the advantages of the bifurcate method of classification, which had been suggested to him by a chapter in Saunderson's logic and the diagram given in the college course. Although the Tree of Porphyry and the principles of bifurcation had been mentioned by almost all logicians, the utility and excellence of the method, he says (p. 287), had not made itself apparent. Indeed the method was mentioned but to be slighted, or to be made a subject of pleasantry by Reid and Kames. Bentham sufficiently states his own opinion when he speaks (p. 295) of 'the matchless beauty of the Ramean Tree.' After fully showing its logical value as an exhaustive method of classification, and refuting the objections of Reid and Kames, on a wrong ground, as I think, he proceeds to inquire to what length it may be carried. He correctly points out two objections to the extensive use of bifid arrangements, (1) because they soon become impracticably extensive and unwieldy, and (2) because they are uneconomical. In his day the recorded number of different species of plants was 40,000, and he leaves the reader to estimate the immense number of branches and the enormous area of a bifurcate table which should exhibit one scheme. He also points out the apparent loss of labour in making any large bifurcate classification; but this he considers to be fully recompensed by the logical value of the result, and the logical training acquired in its execution. Jeremy Bentham, then, fully recognises, as I conceive, the value of the Logical Abecedarium under another name, though he apprehends the limit to its use placed by the finiteness of our mental and manual powers.

Mr. George Bentham has also fully recognised the value of bifurcate classification, both in his 'Outline of a New System of Logic'[u] (pp. 105–118), and in his 'Essai sur la Nomenclature et la Classification.' This latter work consists of a free translation or improved version in French of Jeremy Bentham's 'Essay on Classification.' Further illustrations of the value of the bifurcate method are adduced from the natural sciences, and Mr. Bentham points out that it is really this method which was employed by Lamark and Decandolle in their so-called analytical arrangement of the French Flora. The following table contains an excellent example of bifurcate division, consisting of the principal classes of Decandolle's system, as given by Mr. Bentham in Table No. III. p. 108 of his Essay, the names, however, being translated:—

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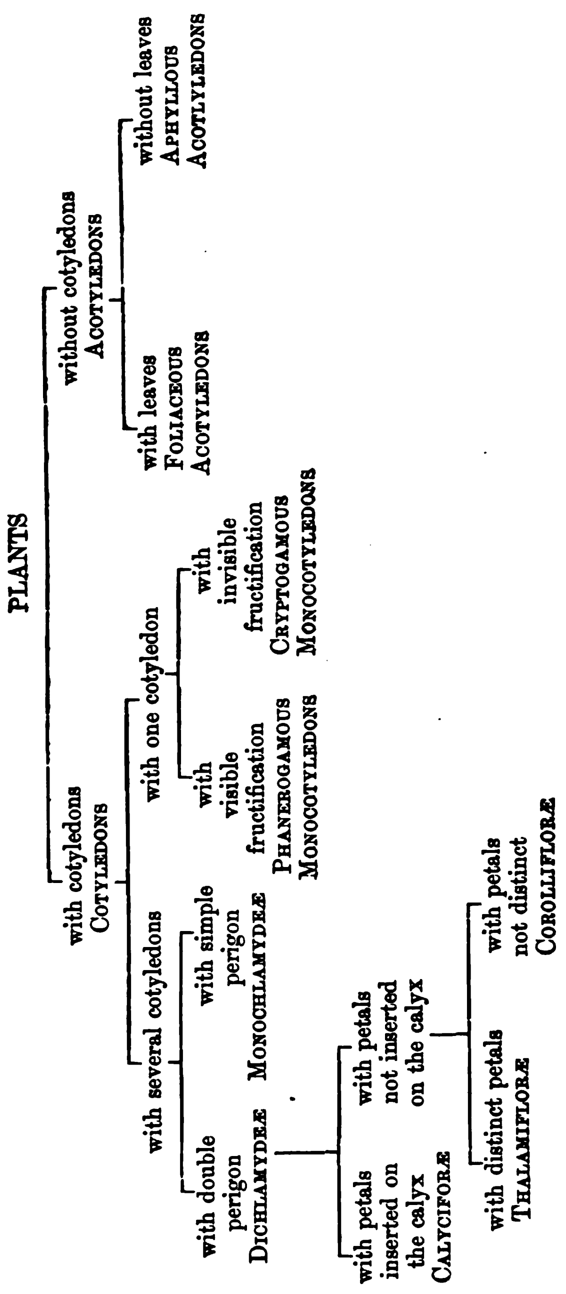
[u] Concerning the connexion of this work with the great discovery of the quantification of the predicate, I may refer the reader to the remarks and articles of Mr. Herbert Spencer and Professor Thomas Spencer Baynes, in the 'Contemporary Review' of March, April, and July, 1873, vol. xxi. pp. 490, 796; vol. xxii. p. 318; as also to my own article in answer to Professor Baynes in the same Review for May, 1873, vol. xxi. p. 821. Professor Baynes makes it evident that, when Sir W. Hamilton reviewed Mr. Bentham's work in 1833, he did not sufficiently acquaint himself with its contents. I must continue to hold that the principle of quantification is explicitly stated by Mr. Bentham, and it must be regarded as a remarkable fact in the history of logic that Hamilton, while vindicating, in 1847, his own claims to originality and priority against the scheme of De Morg [illegible] rlooked the much earlier and more cl [illegible]


PLANTS
with cotyledons
COTYLEDONS
without cotyledons
ACOTYLEDONS
with several cotyledons
with one cotyledon
with leaves
FOLIACEOUS
ACOTYLEDONS
without leaves
APHYLLOUS
ACOTYLEDONS
with double
perigon
DICHLAMYDEÆ
with simple
perigon
MONOCHLAMYDEÆ
with
visible
fructification
PHANEROGAMOUS
MONOCOTYLEDONS
with
invisible
fructification
CRYPTOGAMOUS
MONOCOTYLEDONS
with petals
inserted on
the calyx
CALYCIFORÆ
with petals
not inserted
on the calyx
with distinct petals
THALAMIFLORÆ
with petals
not distinct
COROLLIFLORÆ

Mr. Bentham also gives a bifurcate arrangement of animals after the method proposed by Duméril in his 'Zoologie Analytique,' this naturalist being distinguished by his clear perception of the logical importance of the method.

A more recent binary classification of the animal kingdom as regards the larger classes may be found in Professor Reay Greene's 'Manual of the Cœlenterata,' p. 18.

Does Abstraction imply Generalization?

Before we can acquire a sound comprehension of the subject of classification we must answer a very difficult question, namely, whether logical abstraction does or does not always imply generalization. It comes to exactly the same thing if we ask whether a species may be coextensive with its genus, or whether, on the other hand, the genus must contain more than the species. To abstract logically is, as we have seen (vol. i. p. 33), to overlook or withdraw our notice from some point of difference. Whenever we form a class we abstract, for the time being, the differences of the objects so united in respect of some common quality. If, for instance, we class together a great number of objects as dwelling-houses, we overlook or abstract the fact that some dwelling-houses are constructed of stone, others of brick, wood, iron, &c. Very often at least the abstraction of a circumstance increases the number of objects included under a class according to the law of the inverse relation of the quantities of extension and intension (vol. i. p. 32). Dwelling-house is a wider term than brick dwelling-house. House, or building, is more general still than dwelling-house. But the question before us is, whether abstraction *always* increases the number of objects included in a class, which amounts to asking whether the law of the inverse relation of logical quantities is *always* true. The interest of the question partly arises from the fact, that so high a philosophica authority as Mr. Herbert Spencer has denied that generalization is implied in abstraction[x], making this doctrine the ground for rejecting previous methods of classifying the sciences, and for forming an ingenious but peculiar method of his own. The question is also a fundamental one of the highest logical importance, and involves subtle difficulties which have made me long hesitate in forming a decisive opinion.

Let us attempt to answer the question by examination of a few examples. Compare the two classes *gun* and *iron gun.* It is certain that there are many guns which are not made of iron, so that abstraction of the circumstance 'made of iron' increases the extent of the notion. Next compare *gun* and *metallic gun.* All guns made at the present day consist, I believe, of metal, so that the two notions seem to be co-extensive; but guns were at first made of pieces of wood bound together like a tub, and as the logical term *gun* takes no account of time, it must include all guns that have ever existed. Here again extension increases as intension decreases. Compare once more 'steam-locomotive engine' and 'locomotive engine.' In the present day so far as I am aware all locomotives are worked by steam, so that the omission of that qualification might seem not to widen the term; but it is quite possible that in some future age a different motive power may be used in locomotives; and as there is no limitation of time in the use of logical terms, we must certainly assume that there is a class of locomotives not worked by steam, as well as a class that is worked by steam. When the natural class of Euphorbiaceæ was originally formed, all the plants known to belong to it were devoid of corollas; it would have seemed therefore that the two classes 'Euphorbiaceæ,' and 'Euphorbiaceæ devo of Corollas,' were of equal extent. Subsequently a number of plants plainly belonging to the same class were found in tropical countries, and they possessed bright coloured corollas. Naturalists believe with the utmost confidence that 'Ruminants' and 'Ruminants with cleft feet' are identical terms, because no ruminant has yet been discovered without cleft feet. But we can see no impossibility in the conjunction of rumination with uncleft feet, and it would be too great an assumption to say that we are certain that an example of it will never be met with. Instances can be quoted, without end, of objects being ultimately discovered which combined properties or forms which had never before been seen together. In the animal kingdom the Black Swan, the Ornithorhyncus Paradoxus, and more recently the singular fish called Ceratodus Forsteri, all discovered in Australia, have united characters never previously known to co-exist. At the present time deep-sea dredging is bringing to light many animals of a new and unprecedented nature. Singular exceptional discoveries may certainly occur in other branches of science. When Davy first succeeded in eliminating metallic potassium, it was a well established empirical law that all metallic substances possessed a high specific gravity, the least dense of all metals then known being zinc, of which the specific gravity is 7·1. Yet, to the surprise of chemists, potassium was found to be an undoubted metal of less density than water, its specific gravity being 0·865.

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[x] 'The Classification of the Sciences,' &c., 3rd edit.]

It is hardly requisite to prove by further examples that our knowledge of nature is incomplete, so that we cannot safely assume the non-existence of new combinations. Logically speaking, we ought to leave a place open for animals which ruminate but are without cleft feet, and for every other possible intermediate form of animal, plant, or mineral. A purely logical classification must take account not o [illegible] does exist, but of what may in af [illegible]

I will go a step further, and say that we must have places in our scientific classifications for purely imaginary existences. A very large proportion of the mathematical functions which are conceivable have no application to the circumstances of this world. Physicists certainly do investigate the nature and consequences of forces which nowhere exist. Newton's 'Principia' is full of such investigations. In one chapter of his 'Mécanique Céleste' Laplace indulges in a remarkable speculation as to what the laws of motion would have been if momentum instead of varying simply as the velocity had been a more complicated function of it. I have already mentioned (vol. i. p. 256) that Sir George Airy contemplated the existence of a world in which the laws of force should be such that a perpetual motion would be possible, and the Law of Conservation of Energy would not hold true.

Thought is not bound down to the limits of what is materially existent, but is circumscribed only by those Fundamental Laws of Identity, Contradiction and Duality, which were laid down at the outset. This is the point at which I should differ from Mr. Herbert Spencer. He appears to suppose that a classification is complete if it has a place for every existing object, and this may perhaps seem to be practically sufficient; but it is subject to two profound objections. Firstly, we do not know all that exists, and therefore in limiting our classes we are erroneously omitting multitudes of objects of unknown form and nature which may exist either on this earth or in other parts of space. Secondly, as I have explained, the powers of thought are not limited by material existences, and we may or, for some purposes, must imagine objects which probably do not exist, and if we imagine them we ought (strictly speaking) to find appropriate places for them in the classifications of science.

The chief difficulty of this subject, however, consists in the fact that mathematical or other certain laws may entirely forbid the existence of some combinations. The circle may be defined as a plane curve of equal curvature, and it is a property of it that it contains the greatest area within the least possible perimeter. May we then contemplate mentally a circle not a figure of greatest possible area? Or, to take a still simpler example, a parallelogram possesses the property of having the opposite angles equal. May we then mentally divide parallelograms into two classes according as they do or do not have their opposite angles equal? It might seem absurd to do so, because we know that one of the two species of parallelogram would be non-existent. But, then, what is the meaning of the thirty-fourth proposition of Euclid's first book, unless the student had previously contemplated the existence of both species as possible. We cannot even deny or disprove the existence of a certain combination without thereby in a certain way recognising that combination as an object of thought.

The general conclusion, then, at which I arrive, is in opposition to that of Mr. Herbert Spencer. I think that whenever we abstract a quality or circumstance we do generalize or widen the notion from which we abstract. Whatever the terms A, B, and C may be, I hold that in strict logic AB is mentally a wider term than ABC, because AB includes the two species ABC and AB*c*. The term A is wider still, for it includes the four species ABC, AB*c*, A*b*C, A*bc*. The Logical Abecedarium, in short, is the only limit of the classes of objects which we must contemplate in a purely logical point of view. Whatever notions be brought before us, we must mentally combine them in all the ways sanctioned by the laws of thought and exhibited in the Abecedarium, and it is a matter for after consideration to determine how many of these combinations exist in outward nature, or how many are actually forbidden by the nature of space. A classification is essentially a mental not a material thing.

Discovery of Marks or Characteristics.

Although the chief purpose of classification is to disclose the deepest and most general resemblances of the objects classified, yet the practical value of any particular system will partly depend upon the ease with which we can refer an object to its proper class, and thus infer concerning it all that is known generally of that class. This operation of discovering to which class of a system a certain specimen or case belongs is generally called *Diagnosis*, a technical term very familiarly used by physicians, who constantly require to diagnose or determine the nature of the disease from which a patient is suffering. Now every class is defined by certain specified qualities or circumstances, the whole of which are present in every object contained in the class, and not all present in any object excluded from it. These defining circumstances ought to consist of the deepest and most important circumstances, by which we vaguely mean those probably forming the conditions with which the minor circumstances are correlated. But it will often happen that the so-called important points of an object are not those which can most readily be observed. Thus the two great classes of phanerogamous plants are defined respectively by the possession of two cotyledons or seed-leaves, and one cotyledon. But when a plant comes to our notice and we want to refer it to the right class, it will often happen that we have no seed at all to examine, in order to discover whether there be one seed-leaf or two in the germ. Even if we have a seed it will often be very sm[illegible] and a careful dissection under the microscope will be r[illegible] [illegible] ascertain the number of cotyle[illegible] [illegible] examination of the germ would mislead us, for the cotyledons may be obsolete, as in Cuscuta, or united together, as in Clintonia. Botanists therefore seldom actually refer to the seed for such simple information. Certain other characters of a plant are closely correlated with the number of seed-leaves; thus monocotyledonous plants almost always possess leaves with parallel veins like those of grass, while dicotyledonous plants have leaves with reticulated veins like those of an oak leaf. In monocotyledonous plants, too, the parts of the flower are most often three or some multiple of three in number, while in dicotyledonous plants the numbers four and five and their multiples prevail. Botanists, therefore, by a glance at the leaves and flowers can almost certainly refer a plant to its right class, and can infer not only the number of cotyledons which would be found in the seed or young plant, but also the structure of the stem and the other general characters and relations of a dicotyledon or a monocotyledon.

Any conspicuous and easily discriminated property which we thus select for the purpose of deciding to which class an object belongs, may be called a *characteristic*. The logical conditions of a good characteristic mark are very simple, namely, that it should be possessed by all objects entering into a certain class, and by none others. The characteristic may consist either of a single quality or circumstance, or of a conjunction of such, provided that they all be constant and easily detected. Thus in the classification of mammals the teeth are of the greatest assistance, not because a slight variation in the number and form of the teeth is of any great importance in the general economy of the animal, but because such variations are found [illegible] irical observation to coincide with most im[illegible] he general affinities. It is found [illegible] genera of mammals can be registered and discriminated accurately by their teeth, especially by the foremost molars and the hindmost premolars. Some of the teeth, indeed, are occasionally missing, so that zoologists prefer to trust to those characteristic teeth which are most constant[y], and to infer from them not only the arrangement of the other teeth, but the whole conformation of the animal.

It is a very difficult matter to mark out any boundary-line between the animal and vegetable kingdoms, and it may even be doubted whether any rigorous division can be established. The most fundamental and important character of a vegetable structure probably consists in the absence of nitrogen from the constituent membranes. Supposing this to be the case, the difficulty arises that in examining minute organisms we cannot ascertain directly whether they contain nitrogen or not. Some minor but easily detected circumstance is therefore needed to discriminate between animals and vegetables, and this is furnished to some extent by the fact that the production of starch granules is restricted to the vegetable kingdom. Thus the Desmidiaceæ may be safely assigned to the vegetable kingdom, because they contain starch. But we must not employ this characteristic negatively; the Diatomaceæ are probably vegetables, though they do not produce starch.

Diagnostic Systems of Classification.

We have seen that diagnosis is the process of discovering the place in any system of classes, to which an object has already been referred by some previous investigation, the object being to avail ourselves of the information concerning such an object whi[illegible] been already accumulated and recorded. It matter of the greatest importance, for, unless we can recognise, from time to time, objects or substances which have been before investigated, all recorded discoveries would lose their value. Even a single investigator must have some means of recording or systematizing his observations of any large number of objects like those furnished by the vegetable and animal kingdoms.

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[y] Owen, 'Essay on the Clas
the Mammalia,' p. 20.

Now whenever a class has been properly formed, a definition must have been laid down, stating the qualities and circumstances possessed by all the objects which are intended to be included in the class, and not possessed completely by any other objects. Diagnosis, therefore, consists simply in comparing the qualities of a certain object with the definitions of a series of classes; the absence in the object of any one quality stated in the definition excludes it from the class thus defined; whereas, if we find every point of a definition exactly fulfilled in the specimen, we may at once assign it to the class in question. It is of course by no means certain that everything which has been affirmed of a class is true of all objects afterwards referred to the class; for this would be a case of imperfect inference, which is never more than a matter of probability. A definition can only make known a finite number of the properties of an object, so that it always remains possible that objects agreeing in those assigned properties will differ in other ones. An individual cannot be defined, and can only be made known by the exhibition of the individual itself, or by a material specimen exactly representing it. But this and many other questions relating to definition must be treated if I am able to take up the general subject of language in another work.

[illegible]c systems of classification should, as a general [illegible]ged on the bifurcate method explicitly. Any [illegible] be chosen which divides the whole group of objects into two distinct parts, and each part may be sub-divided successively by any prominent and well marked circumstance which is present in a large part of the genus and not in the other. To refer an object to its proper place in such an arrangement we have only to note whether it does or does not possess the successsive critical circumstances. Dana devised a classification of this kind[z] by which to refer any crystal to its place in the series of six or seven classes already described. If a crystal has all its edges modified alike or the angles replaced by three or six similar planes, it belongs to the monometric system; if not, we observe whether the number of similar planes at the extremity of the crystal is three or some multiple of three, in which case it is a crystal of the hexagonal system; and so we proceed with further successive discriminations.

To ascertain the name of a mineral by examination with the blow-pipe, an arrangement more or less evidently on the bifurcate plan, has been laid down by Von Kobell[a]. Minerals are divided according as they possess or do not possess metallic lustre; as they are fusible (including under fusible substances those which are volatile) or not fusible in a determinate degree, according as they do or do not on charcoal give a metallic bead, and so on.

Perhaps the best example to be found of any arrangement simply devised for the purpose of diagnosis, is Mr. George Bentham's 'Analytical Key to the Natural Orders and Anamolous Genera of the British Flora,' given in his 'Handbook of the British Flora[b].' In this scheme, the great composite family of plants, together with the closely approximate genus Jasione, are first separated from all other flowering plants by the compound character of their flowers. The remaining plants are subdivided according as the perianth is double or single. Since no plants are yet known in which the perianth can be said to have three or more distinct rings, this division becomes practically the same as one into double and not-double. Flowers with a double perianth are next discriminated according as the corolla does or does not consist of one piece, according as the ovary is free or not-free, as it is simple or not simple, as the corolla is regular or irregular, and so on. On looking over this arrangement, it will be found that numerical discriminations often occur, the numbers of petals, stamens, capsules, or other parts being the criteria, in which cases, as already explained (vol. ii. p. 374), the actual exhibition of the bifid division would be tedious.

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[z] Dana's 'Mineralogy,' vol. i. p. 123. Quoted in Watts's 'Dictionary of Chemistry,' vol. ii. p. 166.

[a] 'Instructions for the Discrimination of Minerals by Simple Chemical Experiments,' by Franz von Kobell, translated [illegible] by R. C. Campbell, Glasgow, 1841.

[b] Edition of 1866, p. lxiii.

Linnæus appears to have been perfectly acquainted with the nature and uses of diagnostic classification, which he describes under the name of Synopsis, saying[c] :—'Synopsis tradit Divisiones arbitrarias, longiores aut breviores, plures aut pauciores : a Botanicis in genere non agnoscenda. Synopsis est dichotomia arbitraria, quæ instar viæ ad Botanicem ducit. Limites autem non determinat.'

The rules and tables drawn out by chemists to facilitate the discovery of the nature of a substance in qualitative analysis are usually arranged on the bifurcate method, and form excellent examples of diagnostic classification, the qualities of the substances employed in testing being in most cases merely characteristic properties of little importance in other respects. The chemist does not detect [illegible] ıg it to the state of metallic potas-

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[illegible] nica' (1770), § 154. p. 98.

sium, and then observing whether it has all the principal qualities belonging to potassium. He selects from among the whole number of compounds of potassium that salt, namely the compound of platinum tetra-chloride and potassium chloride, which has the most distinctive appearance, as it is comparatively insoluble and produces a peculiar yellow and highly crystalline precipitate. Accordingly whenever this precipitate can be produced by adding platinum chloride to a solution potassium is present. The fine purple or violet colour which potassium salts usually communicate to the blowpipe flame, had long been used as a characteristic mark. Some other elements were readily detected by the colouring of the blowpipe flame, barium giving a pale yellowish green, and salts of strontium a bright red. By the use of the spectroscope the coloured light given off by any incandescent vapour is made to give perfectly characteristic marks of the elements contained in the vapour.

Diagnosis seems to be identical with the process termed by the ancient logicians *abscissio infiniti*, the cutting off of the infinite or negative part of a classification when we discover by observation that an object possesses a particular property. At every step in a bifurcate division, some objects possessing the difference will fall into the affirmative part or species; all the remaining objects in the world fall into the negative part which will be infinite in extent. Diagnosis consists in the successive rejection from further notice of those almost infinite classes with which the specimen in question does not agree.

Index Classification

Under the gen[illegible] [illegible]bject of [illegible] [illegible]ion we may certainly include [illegible] [illegible]ements [illegible] or nam[illegible] which we make fo[illegible] [illegible]se of [illegible] [illegible]or in th[illegible] discovery of an object. Even such apparently trivial and arbitrary arrangements as alphabetical or other indices, are really classifications subject to all the principles of the subject. No such arrangement can be of any use unless it involves some correlation of circumstances, so that knowing one thing we learn another. If we merely arrange letters in the pigeon-holes of a secretaire we establish a correlation, for all letters in the first hole will be written by persons, for instance, whose names begin with A, and so on. Knowing then the initial letter of the writer's name we know also the place of the letter, and the labour of search is thus reduced to one twenty-sixth part of what it would be without any arrangement.

Now the purpose of a mere catalogue is to discover the place in which an object is to be found, but the art of cataloguing involves logical considerations of some interest and importance. We want to establish a correlation between the place of an object and some circumstance about the object which shall enable us readily to refer to it; this circumstance therefore should be that which will most readily dwell in the memory of the searcher. A piece of poetry, for instance, will be best remembered, in all probability, by the first line of the piece, according to the laws of the association of ideas, and the name of the author will be the next most definite circumstance; a catalogue of poetry should therefore be arranged alphabetically according to the first word of the piece, or the name of the author, or, still better, in both ways. It would be wholly absurd and impossible to arrange poems according to their subjects, so vague and mixed are these found to be when the attempt is made.

It is a matter of considerable literary importance to upon the best mode of cataloguing books, so that quired book in a library shall be most readily Books may be classified in a great number of ways, according to subject, language, date or place of publication, size, the initial words of the book itself, of the title-page, the colophon, the author's name, the publisher's name, the printer's name, the character of the type, and so on. Every one of these modes of arrangement may be useful, for we may happen to remember one circumstance about a book when we have forgotten all others; but as we cannot usually go to the expense of forming more than two or three indices at the most, we must of course select those circumstances for the basis of arrangement which will be likely to lead to the discovery of a book most surely. Many of the criteria mentioned are evidently inapplicable. The language in which a book is written is no doubt definite enough, but would afford no criterion for the classification of any large group of English books, or of those written in any one language. Classification by subjects would be an exceedingly useful method if it were practicable, but experience, or indeed a little reflection, shows it to be a logical absurdity. It is a very difficult matter to classify the sciences, so close and complicated are in many cases the relations between them. But with books the complication is infinitely greater, since the same book may treat successively of different sciences, or it may discuss a problem involving many entirely diverse principles and branches of knowledge. A good history of the steam engine will be antiquarian, so far as it traces out records of the earliest efforts at discovery; purely scientific, as regards the principles of thermodynamics involved; technical, as regards the mechanical means of applying those principles; economical, as regards the industrial results of the invention; biographical, as regards the lives of the inventors. A history of Westminster Abbey might belong either to the history of architecture, the history of the church, or the history of England. If we abandon the attempt to carry out an arrangement according to the natural classification of the sciences, and form comprehensive practical groups, we shall be continually perplexed by the occurrence of intermediate cases, and opinions will differ *ad infinitum* as to the details. If, to avoid the difficulty about Westminster Abbey, we form a class of books devoted to the History of Buildings, the question will then arise whether Stonehenge is a building, and if so, whether, cromlechs, mounds, or even monoliths are so. At the other end of the scale we shall be uncertain whether to include under the class History of Buildings, lighthouses, monuments, bridges, &c. In regard to purely literary works, rigorous classification is still less possible. The very same work may partake of the nature of poetry, biography, history, philosophy, or if we form a comprehensive class of Belles-Lettres, nobody can say exactly what does or does not come under the term.

My own experience entirely bears out the opinion of the late Professor De Morgan, that classification according to the name of the author is the only one practicable in a large library, and this method has been admirably carried out in the great Catalogue of the British Museum. The name of the author is the most precise circumstance concerning a book, which usually dwells in the memory. It is more nearly a characteristic of the book than anything else. In an alphabetical arrangement we have an exhaustive classification, including a place for every possible name. The following remarks[d] of De Morgan seem therefore to be entirely correct. 'From much, almost daily use, of catalogues for many years, I am perfectly satisfied that a classed catalogue is more difficult to use than to make. It is one man's theory of the subdivision of knowledge, and the chances are against its suiting any other man. Even if all doubtful works were entered under several different heads, the frontier of the dubious region would itself be a mere matter of doubt. I never turn from a classed catalogue to an alphabetical one without a feeling of relief and security. With the latter I can always, by taking proper pains, make a library yield its utmost; with the former I can never be satisfied that I have taken proper pains, until I have made it, in fact, as many different catalogues as there are different headings, with separate trouble for each. Those to whom bibliographical research is familiar, know that they have much more frequently to hunt an author than a subject: they know also that in searching for a subject, it is never safe to take another person's view, however good, of the limits of that subject with reference to their own particular purposes.'

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[d] 'Philosophical Magazine,' 3rd Series (1845), vol. xxvi. p. 522.

It is often very desirable, however, that an alphabetical name catalogue should be accompanied by a subordinate subject catalogue, but in this case no attempt should be made to devise a theoretically complete classification. Every principal subject treated in a book should be entered separately in an alphabetical list, under the name most likely to occur to the searcher, or under several names. This method was partially carried out in Watts's valuable 'Bibliotheca Britannica,' but it was perfectly applied in the admirable subject index to the 'British Catalogue of Books,' and equally well in the 'Catalogue of the Manchester Free Library at Campfield,' this latter being the most perfect model of a printed catalogue with which I am acquainted. The public Catalogue of the British Museum is arranged as far as possible according to the alphabetical order of the author's names, but in writing the titles for this catalogue several copies are simultaneously produced by a manifold writer, so that a catalogue according to the order of the books on the shelves, and another according to the first words of the title-page, are created by a mere re-arrangement of the spare copies. In the 'English Cyclopædia' it is suggested that twenty copies of the book titles might readily have been utilized in forming additional catalogues, arranged according to the place of publication, the language of the book, the general nature of the subject, and so forth[e].

It will hardly be a digression to point out the enormous saving of labour, or, what comes to the same thing, the enormous increase in our available knowledge, both literary and scientific, which arises from the formation of extensive indices. The 'State Papers,' containing the whole history of the nation, were practically sealed to literary inquirers until the Government undertook the task of calendaring and indexing them. The British Museum Catalogue is another national work, of which the importance in advancing knowledge cannot be overrated. The Royal Society is accomplishing a work of world-wide importance, in publishing a complete catalogue of memoirs upon physical science. The time will perhaps come when our views upon this subject will be extended, and either Government or some public society will undertake the systematic cataloguing and indexing of masses of historical and scientific information which are now almost closed against inquiry.

Classification in the Biological Sciences.

The great generalizations established in the works of Herbert Spencer and Charles Darwin have thrown great light upon many other sciences, and, strange as it may seem to say so, they have removed several difficulties out of the way of the logician. The subject of classification has long been studied in almost exclusive reference to the same kind of natural classification which is best in biology will apply also in mineralogy, in chemistry, or in astronomy. The universal logical principles which underlie all classifications are of course the same in natural history as in the sciences of brute matter, but the special logical resemblances which arise from the relation of parent and offspring will not be found to prevail between different kinds of crystals or mineral bodies.

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[e] [illegible] dia,' 'Arts and Sciences,' vol. v. p. 233.

The genealogical view of the mutual relations of animals and plants leads us to discard all notions of any regular progression of living forms, or any theory as to their symmetrical relations. It was at one time a great question whether the ultimate scheme of natural classification would prove to be in a simple line, or a circle, or a combination of circles. Macleay's once celebrated system was a circular one, and each class-circle was composed of five order-circles, each of which was composed again of five tribe-circles, and so on, the subdivision being at each step into five minor circles. Thus he held that in the animal kingdom there were five sub-kingdoms—the Vertebrata, Annulosa, Radiata, Acrita, and Mollusca. Each of these was again divided into five—the Vertebrata consisting of Mammalia, Reptilia, Pisces, Amphibia, and Aves[g]. It is quite evident that in any such symmetrical system the animals were made to suit themselves to the classes instead of the classes being suited to the animals.

We now perceive that the ultimate system will be an almost infinitely extended genealogical tree, which will be capable of representation by lines on a plane surface of sufficient extent. But there is not the least reason to suppose that this tree will have a symmetrical form. Some branches of it would be immensely developed compared with others. In some cases a form may have pro-

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[g] Swainson, 'Treatise on the Geography and Classification of Animals,' 'Cabinet Cyclopædia,' p. 201.

and there will be an immense number of forms finely graduated in their resemblances. Exact and distinct classification will then be almost impossible, and the wisest course will be not to attempt arbitrarily to distinguish forms closely related in nature, but to allow that there exist transitional forms of every degree, to mark out if possible the extreme limits of the family relationship, and perhaps to select the most generalized form, or that which presents the greatest number of close resemblances to others of the family, as the *type* of the whole.

Mr. Darwin, in his most interesting work upon Orchids, points out that the tribe of Malaxeæ are distinguished from Epidendreæ by the absence of a caudicle to the pollinia, but as some of the Malaxeæ have a minute caudicle the division really breaks down in the most essential point.

'This is a misfortune,' he remarks[h], 'which every naturalist encounters in attempting to classify a largely developed or so-called natural group, in which, relatively to other groups, there has been little extinction. In order that the naturalist may be enabled to give precise and clear definitions of his divisions, whole ranks of intermediate or gradational forms must have been utterly swept away: if here and there a member of the intermediate ranks has escaped annihilation, it puts an effectual bar to any absolutely distinct definition.'

In other cases a particular plant or animal may perhaps have transmitted its form from generation to generation almost unchanged, or, what comes to the same result, those forms which diverged in character from the parent stock, may have proved unsuitable to their circumstances, and may have perished sooner or later. We shall then

form standing apart from all others, and

t characters. Occasionally we

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f Orchids,' p. 159.

tion. From time to time the notions of naturalists have been greatly widened, especially in the case of Australian animals and plants, by the discovery of unexpected combinations of organs, and such events must often happen in the future. If indeed the time shall come when all the forms of plants are discovered and accurately described, the science of Systematic Botany will then be placed in a new and more favourable position, as remarked by Alphonse Decandolle[k].

It ought, I think, to be allowed that though the genealogical classification of plants or animals is doubtless the most natural and instructive of all, it is not necessarily the best for all purposes. There may be correlations of properties important for medicinal, or other practical purposes, which do not correspond to the correlations of descent. We must regard the bamboo as a tree rather than a grass, although it is botanically a grass. For legal purposes we may still with advantage continue to treat as fish, the whale, seal, and other cetaceæ. We must class plants together according as they are Arctic, or Alpine, or belong to the temperate, sub-tropical or tropical regions. There may be some causes of likeness apart from hereditary relationship, and in a logical and practical point of view we must not attribute exclusive excellence to any one method of classification.

Classification by Types.

Perplexed by the difficulties arising in natural history from the discovery of intermediate forms, naturalists have resorted to what they call classification by types. Instead of [illegible] tinct class defined by the invariabl[illegible]signed properties, and rigidly [illegible] according as they do or those marks, it will then be usually found to include too many forms; if the definition be made more particular, the result is to produce so-called anomalous genera, which, while they are held to belong to the class, do not in all respects conform to its definition. The practice has hence arisen of allowing considerable latitude in the definition of natural orders. The family of Cruciferæ, for instance, forms an exceedingly well marked natural order, and among its characters we find it specified that the fruit is a pod, divided into two cells by a thin partition, from which the valves generally separate at maturity; but we are also informed that, in a few genera, the pod is one-celled, or indehiscent, or separates transversely into several joints[m]. Now this must either mean that the formation of the pod is not an essential point in the definition, or that there are several closely associated families.

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[k] [illegible]e,' p. 16.

The same holds true of typical classification. The type itself is an individual, not a class, and no other object can be exactly like the type. But so soon as we abstract the individual peculiarities of the type and thus specify a finite number of qualities in which other objects may resemble the type, we immediately constitute a class. If some objects resemble the type in some points and others in other points, then each definite collection of points of resemblance constitutes intensively a separate class. The very notion of classification by types is in fact erroneous in a strictly logical point of view. The naturalist is constantly occupied by endeavouring to mark out definite groups of living forms, where the forms themselves do not in many cases admit of any such rigorous lines of demarcation. A certain laxity of logical method is thus apt to creep in, the [illegible] which will be arisen within historical times, and varieties approaching to sub-species may often be produced by the horticulturist in a few years. Such varieties can easily be brought back to their original form, or, if placed in the original circumstances, will themselves revert to that form; but according to Darwin's views all forms are capable of unlimited change, and, it might possibly be, unlimited reversion, if sufficient time and suitable circumstances be granted.

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[m] Bentham's 'Handbo [illegible]

Many fruitless and erroneous attempts have been made to establish some rigorous criterion of specific and generic difference, so that these classes might have a definite value or rank in all branches of biology. Linnæus adopted the view that the species was to be defined as a distinct Creation saying[n], 'Species tot numeramus, quot diversæ formæ in principio sunt creatæ,' or again, 'Species tot sunt, quot diversas formas ab initio produxit Infinitum Ens; quæ formæ, secundum generationis inditas leges, produxere plures, at sibi semper similes.' Of genera he also says[o], 'Genus omne est naturale, in primordio tale creatum.' It was a common doctrine added to and essential to that of distinct creation that these species could not produce intermediate and variable forms, so that we find Linnæus in another work obliged by the ascertained existence of hybrids to take a different view; he says[p], 'Novas species immo et genera ex copula diversarum specierum in regno vegetabilium oriri primo intuitu paradoxum videtur; interim observationes sic fieri non ita dissuadent.' Even supposing in the present day that we could assent to the notion of a certain number of distinct creational acts, this notion would not help us in the theory of classification. Naturalists have never pointed out any separate method of deciding what are the results of distinct creations, and what are not. As Darwin says[q], 'the definition must not include an element which cannot possibly be ascertained, such as an act of creation.' It is, in fact, by investigation of forms and classification that we should ascertain what were distinct creations and what were not; this information would be a result and not a means of classification.

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[n] 'Ph[illegible]nica' (1770), § 157, p. 99.

[o] [illegible]

[p] [illegible] vol. i. p. 70. Quoted in 'Edin- [illegible] pp. 416, 417.

The eminent naturalist Agassiz seems to consider that he has discovered an important principle, to the effect that general plan or structure is the true ground for the discrimination of the great classes of animals, which may be called branches of the animal kingdom[r]. He also thinks that genera are definite and natural groups. 'Genera,' he says[s], 'are most closely allied groups of animals, differing neither in form, nor in complication of structure, but simply in the ultimate structural peculiarities of some of their parts; and this is, I believe, the best definition which can be given of genera.' But it is surely apparent that there are endless degrees both of structural peculiarity and of complication of structure. It is impossible to define the amount of structural peculiarity which constitutes the genus as distinguished from the species.

The form which any classification of plants or animals tends to take is that of an unlimited series of subaltern classes. Originally botanists confined themselves for the most part to a limited number of such classes; thus Linnæus adopted Class, Order, Genus, Species, and Variety, and even seemed to think that there was something essentially natural in a five-fold arrangement of groups[t].

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[q] 'Descent of Man,' vol. i. p. 228.

[r] Agassiz, 'Essay on Classification,' p. 219.

[s] Ibid. p. 249.

[t] 'Philosophia Botanica,' § 155, p. 98.

With the progress of botany intermediate and additional divisions have gradually been introduced. According to the Laws of Botanical Nomenclature adopted by the International Botanical Congress, held at Paris[u] in August, 1867, no less than twenty-one names of classes are recognised—namely, Kingdom, Division, Sub-division, Class, Sub-class, Cohort, Sub-cohort, Order, Sub-order, Tribe, Sub-tribe, Genus, Sub-genus, Section, Sub-section, Species, Sub-species, Variety, Sub-variety, Variation, Sub-variation. It is allowed by the authors of this scheme, that the definition or degree of importance to be attributed to any of these terms may vary in a certain degree according to individual opinion. The only point on which botanists are not allowed discretion is as to the order of the successive sub-divisions; the division of genera into tribes, or of tribes into orders; any inversion, in short, of the arrangement being inadmissible. There is no reason to suppose that even the above list is complete and inextensible. The Botanical Congress itself recognised the distinction between variations according as they are Seedlings, Half-breeds, or *Lusus Naturæ*. The complication of the inferior classes is increased again by the existence of *hybrids*, arising from the fertilization of one species by another deemed a distinct species, nor can we place any limit to the minuteness of discrimination of degrees of breeding short of an actual pedigree of descent.

It will be evident to the reader that in the remarks upon classification as applied to the Natural Sciences, given in this and the preceding sections, I have not in the least attempted to treat the subject in a manner adequate to its extent and importance. A volume would be insufficient for tracing out the principles of scientific method appears are more numerous than all the other compounds known to chemists. Almost the whole of the substances which have been called organic contain carbon, and are probably held together by the carbon atoms, so that many chemists are now inclined to abandon the name Organic Chemistry, and substitute the name Chemistry of the Carbon Compounds. It used to be believed that the production of the so-called organic compounds was due solely to the action of a vital force, or some inexplicable cause involved in the phenomena of life, but it is now found that chemists are able to commence with the elementary materials, pure carbon, hydrogen, and oxygen, and by strictly chemical operations, combine these together so as to form complicated organic compounds. So many compounds have already been thus formed that the probability is very great that many others will be so formed in the course of time, and we might be inclined to generalize, and infer that all so-called organic compounds might ultimately be produced without the agency of living beings. Thus the distinction between the organic and the inorganic kingdoms seems to be breaking down, but our wonder at the peculiar powers of carbon must increase at the same time.

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[u] 'Laws of Botanical Nomenclature,' by Alphonse Decandolle, translated from the French, 18[illegible]

In considering generalization, the law of continuity was applied chiefly to physical properties capable of mathematical treatment. But in the classificatory sciences, also, the same important principle is often beautifully exemplified. Many objects or events seem to be entirely exceptional and abnormal, and in regard to degree or magnitude they may be so termed. We might adduce examples on the one hand of such extreme cases, but it is often easy to show, on the other hand, that they are connected by intermediate links with other apparently different cases.

In the organic kingdoms of nature there is a common duce a limb, and in appearance there is no analogy. But it was suggested by Blumenbach that the healing of a wound in the higher animals really represents in a lower degree the power of reproducing a limb. That this is true may be shown by adducing a multitude of intermediate cases, each adjoining pair of which are clearly analogous, so that we pass gradually from one extreme to the other. Darwin holds, moreover, that any such restoration of parts is closely connected with that perpetual replacement of the particles which causes every organized body to be after a time entirely new as regards its constituent substance. In short, we approach to a great generalization under which all the phenomena of growth, restoration, and maintenance of organs are effects of one and the same power[x]. It is perhaps still more surprising to find that the complicated process of sexual reproduction in the higher animals may be gradually traced down to a simpler and simpler form, which at last becomes undistinguishable from the budding out of one plant from the stem of another. By a great generalization we may regard all the modes of reproduction of organic life as alike in their nature, and varying only in complexity of development[y].

Limits of Classification.

Science can extend only so far as the power of accurate classification extends. If we cannot detect resemblances, and assign their exact character and amount, we cannot have that generalized knowledge which constitutes science; we cannot infer from case to case. It will readily be

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[x] Darwin, 'The Variation of Animals and Plants,' vol. ii. pp. 293, 359, &c.; quoting Paget, 'Lectures on Pathology,' 1853, pp. 152, 164.

[y] Ibid. vol. ii. p. 372.

The nature of a ray of homogeneous light is strictly defined, either by its place in the spectrum or by the corresponding wave-length, but a ray of mixed light admits of no simple classification; any of the infinitely numerous rays of the continuous spectrum may be present or absent, or present in various intensities, so that we can only class and define a mixed colour by defining the intensity and wave-length of each ray of homogeneous light which is present in it. Complete spectroscopic analysis and the determination of the intensity of every part of the spectrum yielded by a mixed ray is requisite for its accurate classification. Nearly the same may be said of complex sounds. A simple sound undulation, if we could meet with such a sound, would admit of precise and exhaustive classification as regards pitch, the length of wave, or the number of waves reaching the ear per second being a sufficient criterion. But almost all ordinary sounds, even those of musical instruments, consist of complex aggregates of undulations of several different pitches, and in order to classify the sound we should have to measure the intensities of each of the constituent sounds, a work which has been partially accomplished by Professor Helmholtz, as regards the vowel sounds. The different tones of voice distinctive of different individuals must also be due to the intermixture of minute waves of various pitch, which are at present quite beyond the range of experimental investigation. We cannot, then, at present, attempt to classify the different kinds or *timbres* of sound.

The difficulties of classification are even greater when a varying phenomenon cannot be shown to be a mixture of simpler phenomena. If we attempt, for instance, to classify the tastes of natural and artificial substances, we [illegible] group them according as they are sweet, [illegible] aline, acid, astringent, or fiery; but it is [illegible] groups are bounded by no sharp lines be found that states of mind really graduate into each other, so that rigorous classification would prove to be hopeless.

A little reflection will show that there are whole worlds of existences which in like manner are incapable of logical analysis and classification. One friend may be able to single out and identify another friend by his countenance among a million other countenances. Faces are capable of infinite discrimination, but who shall classify and define them, or say by what particular shades of feature he does judge. There are of course certain distinct types of face, but each type is connected with each other type by infinite intermediate specimens. We may classify melodies according to the major or minor key, the character of the time, and some other distinct points; but every melody has independently of such circumstances its own distinctive character and effect upon the mind. Similar remarks might be made concerning a multitude of other circumstances. We can detect differences between the styles of literary, musical, or artistic compositions. We can even in some cases assign a picture to its painter, or a symphony to its composer, by a subtle feeling of resemblances or differences of character and expression, which may be felt, but cannot be described.

Finally, it is apparent that in human character there is unfathomable and inexhaustible diversity. Every mind is more or less like every other mind; there is always a basis of similarity, but there is a superstructure of feelings, impulses, or motives which is distinctive for each person. We can often, indeed, predict the general character of the feelings or actions which will be produced in a given individual well known to us, by a given external event, but we also know that we are often inexplicably at fault in all our inferences. No one can safely generalize upon the subtle variations of temper and emotion which may

BOOK VI.

CHAPTER XXXI.

REFLECTIONS ON THE RESULTS AND LIMITS OF SCIENTIFIC METHOD.

BEFORE concluding a work on the Principles of Science, it will not be inappropriate to add some remarks upon the limits and ultimate bearings of the knowledge which we may acquire by the constant employment of scientific method. All science consists, it has several times been stated, in the detection of identities and uniformities in the action of natural agents. The purpose of inductive inquiry is to ascertain the apparent existence of necessary connexion between causes and effects, the establishment of natural laws. Now so far as we thus learn the invariable course of nature, the future becomes the necessary sequel of the present, and we are brought beneath the sway of powers with which nothing can interfere.

By degrees it is found, too, that the chemistry of organized substances is not widely separated from, but is rather continuous with, that of earth and stones. Life itself seems to be nothing but a special form of that energy which is manifested in heat and electricity and mechanical force. The time may come, it almost seems, when the tender mechanism of the brain will be traced out, and every thought reduced to the expenditure of a determinate weight of nitrogen and phosphorus. No apparent limit exists to the success of scientific method in weighing and measuring, and reducing beneath the sway of law, the phenomena both of matter and of mind. And if mental phenomena be thus capable of treatment by the balance and the micrometer, can we any longer hold that mind is distinct from matter? Must not the same inexorable reign of law, which is apparent in the motions of brute matter, be extended to the most subtle feelings of the human heart? Are not plants and animals and ultimately man himself, merely crystals, as it were, of a complicated form? If so, our boasted Free Will becomes a delusion, Moral Responsibility a fiction, Spirit a mere name for the more curious manifestations of material energy. All that happens, whether right or wrong, pleasurable or painful, is but the outcome of the necessary relations of time and space and force, and of the laws of matter emerging from them, which are fixed in the very nature of things.

Materialism seems, then, to be the coming religion, and resignation to the nonenity of human will the only duty. Such may not generally be the reflections of men of science, but I believe that we may thus describe the secret feelings of fear which the constant advance of scientific investigation excites in the minds of many who view it from a distance. Is science, then, essentially atheistic and materialistic in its tendency? Does the uniform action of material causes, which we learn with an ever increasing approach to certainty, preclude the hypothesis of an intelligent and benevolent Creator, who has not only designed the existing universe, but who still retains the power to alter its course from time to time?

To enter actually upon theological discussions would be evidently beyond the scope of this work. It is with the scientific method common to all the sciences, and not with any of the separate sciences, that we are concerned. Theology therefore would be at least as much beyond my scope as chemistry or geology. But I believe that grave misapprehensions exist as regards the very nature of this scientific method. There are scientific men who assert that the interposition of Providence is impossible, and prayer an absurdity, because the laws of nature are inductively proved to be invariable. Inferences are drawn not so much from particular sciences as from the logical foundations of science itself, to negative the impulses and hopes of men. Now I may properly venture to state that my own studies in logic lead me to call in question all such negative inferences. Those so-called laws of nature are uniformities observed to exist in the action of certain material agents, but it is logically impossible to show that all other agents must behave as these do. The too exclusive study of particular branches of physical science seems in some cases to generate an over confident and dogmatic spirit. Rejoicing in the success with which a few groups of facts are brought beneath the apparent sway of laws, the investigator hastily assumes that he is close upon the ultimate springs of being. A particle of gelatinous matter is found to obey the ordinary laws of chemistry; yet it moves and lives. The world is therefore asked to believe that chemistry can resolve the mysteries of existence.

The Meaning of Natural Law.

Pindar speaks of Law as the Ruler of the Mortals and the Immortals, and it seems to be commonly supposed that the so-called Laws of Nature, in like manner, rule man and his Creator. The course of nature is rega as being determined by invariable principles of mecha which have acted since the world began, and will act infinite ages to come. Even if the origin of all thing attributed to an intelligent creative mind, that Bein regarded as having yielded up arbitrary power, and being subject like a human legislator to the laws w he has himself enacted. Such notions I should desc as superficial and erroneous, being derived, as I th from false views of the nature of scientific inference, the degree of certainty of the knowledge which we acq by inductive investigation.

A law of nature, as I regard the meaning of expression, is not a uniformity which must be obeyed all objects, but merely a uniformity which is as a matt fact obeyed by those objects which have come ben our observation. There is nothing whatever incon tible with logic in the discovery of objects which sh prove exceptions to any law of nature. Perhaps the established law is that which asserts an invariable relation to exist between gravity and inertia, so tha gravitating bodies are found to possess inertia, and bodies possessing inertia are found to gravitate. B would be no reproach to our scientific method, if somet were ultimately discovered to possess gravity withou ertia. Strictly defined and correctly interpreted, the itself would acknowledge the possibility; for with statement of every law we ought properly to join an mate of the number of instances in which it has observed to hold true, and the probability thence ca lated, that it will hold true in the next case. Now a before found (vol. i. p. 299) no finite number of insta can warrant us in expecting with certainty that the instance will be of like nature; in the formulas yie by the inverse method of probabilities a unit al appears to represent the probability that our inference will be mistaken. I demur to the assumption that there is any necessary truth even in such fundamental laws of nature as the Indestructibility of Matter, the Conservation of Force, or the Laws of Motion. Certain it is that men of science have recognised the conceivability of other laws, or even investigated their mathematical conditions. Sir George Airy investigated the mathematical conditions of a perpetual motion (vol. i. p. 256), and Laplace and Newton discussed various imaginary laws of forces inconsistent with those so far observed to operate in the universe (vol. ii. pp. 304, 392).

The laws of nature, as I venture to regard them, are simply general propositions concerning the correlation of properties which have been observed to hold true of bodies hitherto observed. On the assumption that our experience is of adequate extent, and that no arbitrary interference takes place, we are then able to assign the probability, always less than certainty, that the next object of the same apparent nature will conform to the same law.

Infiniteness of the Universe.

We may safely accept as a satisfactory scientific hypothesis the doctrine so grandly put forth by Laplace, who asserted that a perfect knowledge of the universe, as it existed at any given moment, would give a perfect knowledge of what was to happen thenceforth and for ever after. Scientific inference is impossible, unless we may regard the present as the necessary outcome of what is past, and the necessary cause of what is to come. To the view of Perfect Intelligence nothing is uncertain. The astronomer can calculate the positions of the heavenly bodies when thousands of generations of men shall have

passed away, and in this fact we have some illu
tion, as Laplace remarks, of the power which scie
prescience may attain. Doubtless, too, all efforts in
investigation of nature tend to bring us nearer to
possession of that ideally perfect power of intellig
Nevertheless, as Laplace with profound wisdom adds
must ever remain at an infinite distance from the go
our aspirations.

Let us assume, for a time at least, as a highly prob
hypothesis, that whatever is to happen must be the
come of what is; there then arises the question, Wha
Now our knowledge of what exists must ever remain
perfect and fallible in two respects. Firstly, we do
know all the matter that has been created, nor the
manner in which it has been distributed through s
Secondly, assuming that we had that knowledge
should still be wanting in a perfect knowledge of
way in which the particles of matter will act upon
other. The power of scientific prediction extends a
most to the limits of the data employed. Every
clusion is purely hypothetical and conditional upon
non-interference of agencies previously undetected.
law of gravity asserts that every body tends to app
towards every other body, with a certain determ
force, but even supposing the law to hold true, it
not assert that the body *will* approach. No single
nor science can warrant us in making any one abs
prediction. We must know all the laws of nature an
the existing agents acting according to those laws befo
can say what will occur. To assume, then, that scie
method can take everything within its cold embra
uniformity, is to imply that the Creator cannot out
the intelligence of his creatures, and that the exi

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[a] 'Théorie Analytique des Probabilités,' quoted by Babbage, '
Bridgwater Treatise,' p. 173.

universe is not infinite in extent and complexity, an assumption for which I can see no logical basis whatever.

The Indeterminate Problem of Creation.

A second and very serious misapprehension concerning the import of a law of nature may now be pointed out. It is not uncommonly supposed that a law determines the character of the results which shall take place, as, for instance, that the law of gravity determines what force of gravity shall act upon a given particle. Surely a little reflection must render it plain that a law by itself determines nothing. It is a law *plus* agents obeying that law which have results, and it is no part of the law to govern or define the number and place of its own agents. Whether a particle of matter shall gravitate, depends not upon the law of Newton only, but upon the distribution of surrounding particles. The theory of gravitation may perhaps be true throughout all time and in all parts of space, and even the Creator may never find occasion to create those possible exceptions to it which I have asserted to be conceivable. Let this be as it may, and our science cannot certainly determine the question, yet the theory of gravitation itself gives no indication of the forces which may be brought to act at any point of space. The force of gravitation acting upon any particle depends, as we have seen, upon the number, mass, distance, and relative position of all the other particles of matter within the bounds of space at the instant in question. Even assuming that all matter when once distributed through space at the Creation, was thenceforth to act in an invariable manner without subsequent interference, yet the actual configuration of matter at any moment, and the consequent results of the law of gravitation have been entirely a matter of free choice.

Chalmers has most distinctly pointed out tha existing *collocations* of the material world are at as important as the laws which the objects obey. remarks that a certain class of writers entirely look the distinction, and forget that mere laws wi collocations would have afforded no security agai turbid and disorderly chaos[b]. Mr. J. S. Mill has nised[c] the truth of Chalmers' statement, without ing the proper inferences from it. He says[d] of th tribution of matter through space, 'We can di nothing regular in the distribution itself; we can it to no uniformity, to no law.' More lately the of Argyle in his well known work on the 'Reign of has drawn attention to the profound distinction be laws and collocations of causes.

The original conformation of the material univers so far as we can possibly tell, free from all restr There was unlimited space in which to frame it, a unlimited number of material particles, each of could be placed in any one of an infinite num different positions. It must also be added that particle might be endowed with any one of an i number of degrees of *vis viva* acting in any one infinitely infinite number of different directions. problem of Creation was, then, what a mathema would call an indeterminate problem, and it was terminate in an infinitely infinite number of ways. finitely numerous and various universes might have been fashioned by the various distribution original nebulous matter, although all the particles of matter should obey the one law of gravity.

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[b] 'First Bridgwater Treatise' (1834), pp. 16–24.

[c] 'System of Logic,' 5th edit. bk. III. chap. V. § 7. Chap. X

[d] Ibid. vol. i. p. 384.

Lucretius tells us how in the original rain of atoms some of these little bodies diverged from the rectilineal direction, and coming into contact with other atoms gave rise to the various combinations of substances and phenomena which exist. He omitted, indeed, to tell us whence the atoms came, or by what force some of them were caused to diverge, but surely these omissions involve the whole question. I accept the Lucretian conception of creation when properly supplemented. Every atom which existed in any point of space must have existed there previously, or must have been created there by a previously existing Power. When placed there it must have had a definite mass and a definite energy, kinetic or potential as regards other existing atoms. Now, as before remarked, an unlimited number of atoms can be placed in unlimited space in an entirely unlimited number of modes of distribution. Out of infinitely infinite choices which were open to the Creator, that one choice must have been made which has yielded the universe as it now exists.

It would indeed be a mistake to suppose that the law of gravity, when it holds true, is no restriction in the distribution of force. That law is a geometrical law, and it would in many cases be mathematically impossible, as far as we can see, that the force of gravity acting on one particle should be small while that on a neighbouring particle was great. We cannot conceive that even Omnipotent Power should make the angles of a triangle less or greater than two right angles. The primary laws of thought and the fundamental notions of the mathematical sciences do not seem to us to admit of any alteration. Into the metaphysical origin and meaning of the apparent necessity attaching [illegible] I have not fibres, but something requires to be added to our sciences in order that we even explain these subtle phenomena.

Now there is absolutely nothing in science or in scientific method to warrant us in assigning any limit to this hierarchy of laws. When in many undoubted cases we find law overriding law, and at certain points in our experience producing unexpected results, we can never venture to affirm that we have exhausted the strange phenomena which may have been provided for in the original constitution of matter. The Universe might have been so designed that it should for long intervals go through the same round of almost unvaried existence, and yet so that events of exceptional character should from time to time be produced. Charles Babbage showed in that most profound and eloquent work, 'The Ninth Bridgwater Treatise,' that it was theoretically possible for human artists to design a machine, consisting of metallic wheels and levers, which should work invariably by one simple law of action during any finite number of steps, and yet at a fixed moment, however distant, should manifest a single breach of law. Such an engine might go on counting, for instance, the natural numbers until they might reach a number requiring for its expression a hundred million digits. 'If every letter in the volume now before the reader's eyes,' says Babbage*, 'were changed into a figure, and if all the figures contained in a thousand such volumes were arranged in order, the whole together would yet fall far short of the vast induction the observer would have had in favour of the truth of the law of natural numbers . . . Yet shall the engine, true to the prediction of its director, after the lapse of myriads of ages, fulfil its task, and give that one, the first and only exception to that time-sanctioned law. What would have been the chances against the appearance of the excepted case, immediately prior to its oc rence?'

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* 'Ninth Bridgwater Treatise,' p. 140.

As Babbage further showed[f], a calculating eng after proceeding through any required number of moti according to a first law, may be made suddenly to su a change, so that it shall then commence to calcu according to a wholly new law. After giving the nat numbers for any finite time, it might suddenly begi give triangular, or square, or cube numbers, and tl changes might theoretically be conceived as occur time after time. Now if such occurrences can be desig and foreseen by a human artist, it is surely within capacity of the Divine Artist to provide for sim changes of law in the mechanism of the atom, or construction of the heavens.

Physical science, so far as its highest speculations be trusted, gives some indication of a change of law the past history of the Universe. According to Sir Thomson's deductions from Fourier's Theory of H we can trace down the dissipation of heat by conduc and radiation to an infinitely distant time when all thi will be uniformly cold. But we cannot similarly t the heat-history of the Universe to an infinite distanc the past. For a certain negative value of the time formulæ give impossible values, indicating that there some initial distribution of heat which could not have sulted, according to known laws of nature, from any vious distribution[g]. There are other cases in whic consideration of the dissipation of energy leads to conception of a limit to the antiquity of the present o of things[h]. Human science, of course, is fallible, some oversight or erroneous simplification in these theoretical calculations may afterwards be discovered; but as the present state of scientific knowledge is the only ground on which erroneous interpretations of the uniformity of nature and the reign of law are founded, I am right in appealing to the present state of science in opposition to these interpretations. Now the theory of heat places us in the dilemma either of believing in Creation at an assignable date in the past, or else of supposing that some inexplicable change in the working of natural laws then took place. Physical science gives no countenance to the notion of infinite duration of matter in one continuous course of existence. And if in time past there has been a discontinuity of law, why may there not be a similar event awaiting the world in the future. Infinite ingenuity could have implanted some agency in matter so that it might never yet have made its tremendous powers manifest. We have a very good theory of the conservation of energy, but the foremost physicists do not deny that there may possibly be forms of energy, neither kinetic nor potential, and therefore of unknown nature[1].

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[f] 'Ninth Bridgwater Treatise,' pp. 34–43.

[g] Tait's 'Thermodynamics,' p. 38. 'Cambridge Mathematical J nal,' vol. iii. p. 174.

[h] Clerk Maxwell's 'Theory of Heat,' p. 245.

We can imagine reasoning creatures dwelling in a world where the atmosphere was a mixture of oxygen and inflammable gas like the fire-damp of coal mines. If devoid of fire, they might have lived on through long ages in complete unconsciousness of the tremendous forces which a single spark could call into play. In the twinkling of an eye new laws might have come into action, and the poor reasoning creatures who were so confident in their knowledge of the uniform conditions of their world, might have had no time even to speculate upon the overthrow of all their theories. Can we with our finite knowledge be sure that such an overthrow of our theories is impossible?

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[1] Maxwell's 'Theory of Heat,' p. 92.

data show that no appreciable change can be going on, and experimental data to indicate any change are wholly wanting. Geological investigations show indeed that there have been extensive variations of climate in past times; vast glaciers and icebergs have swept over the temperate regions at one time, and tropical vegetation has flourished near the poles at another time. But here again the vicissitudes of climate assume a periodic character, so that the ultimate stability of the earth's condition does not seem to be affected.

All these statements may be reasonable, but they do not in the least establish the Uniformity of Nature in the sense that extensive alterations or sudden catastrophes are impossible. In the first place Laplace's theory of the stability of the planetary system is of an abstract character, as paying regard to nothing but the mutual gravitation of the planetary bodies and the sun. It overlooks several physical causes of change and decay in the system which were not so well known in his day as at present, and it also presupposes the absence of any interruption of the course of things by conflict with foreign astronomical bodies.

It is now commonly acknowledged by astronomers that there are at least two ways in which the *vis viva* of the planets and satellites may suffer loss. The friction of the tides upon the earth produces a small amount of heat which is radiated into space, and this loss of energy must result in a decrease of the rotational velocity, so that ultimately the terrestrial day will become identical with the year, just as the periods of revolution of the moon upon its own axis and around the earth have already become equal. Secondly, there can now be little doubt that various manifestations of electricity upon the earth's surface depend upon the relative motions of the planets and the sun, which give rise to various periods of increased intensity. Such electrical phenomena must result in the production and dissipation c which must be drawn, partially at l moving bodies. This effect is prc have suggested (vol. ii. p. 213), with of energy of comets attributed to medium. But whatever be the thec these phenomena, it is almost certa tendency to the dissipation of the en system, which will in the indefinite in the fall of the planets into the sun

It is hardly probable, however system will be left undisturbed thro period of time required for the dis in this way. Conflict with other being improbable, that it becomes when we take very long intervals As regards cometary conflicts, I am with the negative conclusions drawn display on the evening of the 27th We may often have passed through which are probably electrical ma substantial than the aurora boreali shower of shooting stars may also ceeding from a cometary body, so th have passed through the thinner pa But the earth has probably never pas we have any record, through the nuc consists perhaps of a dense swarm of can only speculate upon the effects duced by such a conflict, but it woul more serious event than any yet The probability of its occurrence assigned ; for though the probabilit one cometary nucleus is almost i number of comets is immensely grea

It is far from impossible, again, that the planetary system may be invaded by bodies of greater mass than any comets. The sun seems to be placed in so extensive a portion of empty space, that its own proper motion would not bring it to the nearest known star (a Centauri) in less than 139,200 years. But in order to be sure that this long interval of undisturbed life is granted to our globe, we must prove that there are no stars moving so as to meet us, and no dark bodies of considerable size flying through intervening space unknown to us. The intrusion of comets into our system, and the fact that many of them have hyperbolic paths, is sufficient to show that the surrounding parts of space are occupied by multitudes of dark bodies of some size. It is quite probable that small suns might have cooled sufficiently to become non-luminous; for even if we discredit the theory that the variation of brightness of periodic stars is due to the revolution of dark companion stars, yet there is our own globe as an unquestionable example of a smaller body which has cooled below the luminous point.

Altogether, then, it is a mere assumption that the Uniformity of Nature involves the unaltered existence of our own globe. There is no kind of catastrophe which is too great or too sudden to be theoretically consistent with the reign of law. For all that our science can tell, human history may be closed in the next instant of time. The world may be dashed to pieces against some intruding body; it may be involved in a nebulous atmosphere of hydrogen to be exploded a second afterwards; it may be scorched up or dissipated into vapour by some great explosion in the sun; there might even be within the globe itself some secret cause of disruption, which only needs time for its manifestation.

There are even some indications, as already noticed (vol. ii. p. 327), that some violent disturbances have apply that name to the passage through each part of space of similar average trains of atoms, the particular successions of atoms being governed only by the theory of probability, and the law of divergence from a mean exhibited in the Arithmetical Triangle. Such a universe would correspond partially to the Lucretian rain of atoms, and to that nebular hypothesis out of which Laplace proposed philosophically to explain the evolution of the planetary system.

According to another extreme supposition, the intense heat energy of this nebulous mass might have been mostly radiated away into the unknown regions of outer space. The attraction of gravity would then have shown itself between each two particles, and the energy of motion thence arising would, by incessant conflicts, be resolved into heat and dissipated.

Inconceivable ages might be required for the completion of this process, but the dissipation of energy thus proceeding could end only in the production of a cold and motionless stone-like universe. The relation of cause and effect, as we see it manifested in life and growth, would then degenerate into the constant existence of every particle in a fixed position relative to every other particle. Logical and geometrical resemblances would still exist between atoms, and between groups of atoms crystallized in their appropriate forms for ever more. But time, the great variable, would bring no variation, and as to human hopes and troubles, they would have come to eternal rest.

Science is not really adequate to proving that such is the inevitable fate of the universe, for we can seldom trust our best established theories and most careful inferences far from their data. Nevertheless, the most probable speculations which we can form as to the history, especially of our own planetary system, is that it origimately fixed by scientific inference. It is progressing towards a state in which the available energy of matter will be dissipated through infinite surrounding space, and all matter will become cold and lifeless. This constitutes, as it were, the historical period of physical science, that over which our scientific insight may more or less extend. But in this, as in other cases, we have no right to interpret our experience negatively, so as to infer that because the present state of things began at a particular time, there was no previous existence. It may be that the present period of material existence is but one of an indefinite series of like periods. All that we can see, and feel, and infer, and reason about may be, as it were, but a part of one single pulsation in the existence of the universe.

After Sir W. Thomson had pointed out the preponderating tendency which now seems to exist towards the conversion of all energy into heat-energy, and its equal diffusion by radiation throughout space, the late Professor Rankine put forth a remarkable speculation[k]. He suggested that the ethereal, or rather, as I have called it, the *adamantine* medium in which all the stars exist, and all radiation takes place, may have bounds, beyond which only empty space may exist. All heat undulations reaching this boundary will be totally reflected, according to the theory of undulations, and will in all probability be reconcentrated into foci situated in many parts of the medium. Whenever a cold and extinct star happens to pass through one of these foci, it will be instantly ignited and resolved by intense heat into its constituent elements. A discontinuity will occur in the history of that portion of matter, and the star will begin its history afresh with a renewed store of energy.

This is doubtless a mere speculation, incapable of verification by observation, and almost free from any strictions afforded by present knowledge. We might at bute various shapes to the whole body of adamant medium, and the consequences would be various. there is this value in such speculations, that they di attention to the finiteness of our knowledge. We can deny the possible truth of such an hypothesis, nor can place a limit to the scientific imagination in the fram of other like hypotheses. It is impossible, indeed, follow out our scientific inferences without falling i speculation. If heat be radiated into outward spac must either proceed *ad infinitum*, or it must be stop somewhere. In the latter case we fall upon Ranki hypothesis. But if the material universe consist of a fi collection of heated matter situated in a finite portion an infinite adamantine medium, then either this unive must have existed for a finite time, or else it must h cooled down during the infinity of past time indefinit near to the absolute zero of temperature. I objected Lucretius' argument against the destructibility of mat that we have no knowledge whatever of the laws acco ing to which it would undergo destruction. But we know the laws according to which the dissipation of h appears to proceed, and the conclusion inevitably is th finite heated material body placed in a perfectly c infinitely extended medium would in an infinite ti become infinitely approximated to zero. Now our c world is not yet cooled down near to zero, so that phys science seems to place us in the dilemma of admitt either the finiteness of past duration of the world, or the finiteness of the portion of medium in which we ex In either case we become involved in metaphysical mechanical difficulties surpassing our mental powers.

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[k] 'Report of the British Association' (1852), Report of Sections, p. 12.

The Divergent Scope for New Discovery.

In the writings of some recent philosophers, especially of Auguste Comte, and in some degree John Stuart Mill, there is an erroneous and hurtful tendency to represent our knowledge as assuming an approximately complete character. At least these and many other writers fail to impress upon their readers a truth which I think cannot be too constantly borne in mind, namely, that the utmost successes which our scientific method can accomplish will not enable us to comprehend more than an infinitesimal fraction of what there doubtless is to comprehend. Professor Tyndall seems to me open to the same charge in a less degree. He remarks[1] that we can probably never bring natural phenomena completely under mathematical laws, because the approach of our sciences towards completeness may perhaps be asymptotic, so that however far we may go, there may still remain some facts not subject to scientific explanation. He thus likens the supply of novel phenomena to a convergent series, the earlier and larger terms of which have been successfully disposed of, so that only comparatively minor groups of phenomena remain for future investigators to occupy themselves upon. On the contrary, as it appears to me, the supply of new and unexplained facts is divergent in extent, so that the more we have explained, the more there is to explain. The further we advance in any generalization, the more numerous and intricate are the exceptional cases still demanding further treatment. The experiments of Boyle, Mariotte, Dalton, Gay-Lussac, and others, upon the physical properties of gases might seem to have exhausted that subject by showing that all gases obey the

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[1] 'Fragments of Science,' p. 362.

same laws as regards temperature, pressure, and volun
But in reality these laws are only approximately tr
and the divergences have afforded a wide and yet qu
unexhausted field for further generalization. The m
recent discoveries of Cagniard de la Tour and Profes
Andrews might seem to have summed up many of the
exceptional facts under a wider generalization, but
reality they have opened to us vast new regions of i
teresting inquiry, and they leave wholly untouched t
question why one gas or one substance behaves different
from another.

The science of Crystallography is that perhaps in whi
the most precise and general laws have been detected, b
it would be utterly untrue to assert that it has lessen
the area of future discovery. We can show that each o
of the seven or eight hundred forms of calcite is deriva
by plain geometrical modifications from an hexago
prism, but who has attempted to explain the molecul
forces producing these modifications, or the chemical co
ditions in which they arise? The law of isomorphism
an important generalization, for it establishes a gene
resemblance between the forms of crystallization of natu
classes of elements. But if we examine a little mo
closely we find that these forms are only approximate
alike, and the divergence peculiar to each substance is
unexplained exception.

By many similar illustrations it might be readily show
that in whatever direction we extend our investigatio
and successfully harmonize a few facts, the result is on
to raise up a host of other unexplained facts. Can a
scientific man venture to state that there is less openi
now for new discoveries than there was three centuri
ago? Is it not rather true that we have but to open
scientific book and read a page or two, and [illegible] ll
probability come to some recorded

no precise explanation can yet be given? In every such fact there is a possible opening for new discoveries, and it can only be the fault of the investigator's mind if he can look around him and find no scope for the exercise of his faculties.

The Infinite Incompleteness of the Mathematical Sciences.

There is one privilege which a certain amount of knowledge should confer; it is that of becoming aware of the indefinite weakness of our powers compared with the tasks which they might undertake if stronger. To the poor savage who cannot count twenty, the arithmetical accomplishments of the ordinary schoolboy are miraculously great in comparison. The schoolboy cannot comprehend the almost infinitely greater powers of the student, who has acquired facility with algebraic processes. The student can but look with feelings of surprise and reverence at the powers of a Newton or Laplace. But the question at once suggests itself, Do the powers of the highest human intellect bear any moderate ratio to the things which are to be understood and calculated? How many further steps must we take in the rise of mental ability and the extension of mathematical method before we begin to exhaust the knowable?

I am inclined to find fault with mathematical writers because they often exult in what they can accomplish, but omit to point out that what they do is but an indefinitely, nay an infinitely, small part of what might be done. They exhibit a general inclination, with few exceptions, not to do so much as mention the existence of problems of an impracticable character. This may be excusable so far as the immediate practical result of their researches is in question, but the custom has the effect of misleading the general public into the fallacious notion that mathem is a *perfect* science, which accomplishes what it un takes in a complete manner. On the contrary, it ma said that if a mathematical problem were selected by chance out of the whole variety which might be propo the probability is infinitely slight that a human ma matician could solve it. Just as the numbers we can c or frame to the mind are literally nothing compared the numbers which might exist, so the whole accompl ments of a Laplace or a Lagrange are, as it were, little corner of the multiplication table, which has re an indefinite extent.

I have sufficiently pointed out that the rude chara of all our observations prevents us from being awar the existence of the greater number of effects and act of nature. It must be added that, if we perceived th we should usually be incapable of including them in theories from want of mathematical power. Some per may be surprised that though nearly two centuries elapsed since the time of Newton's discoveries, we yet no general theory of molecular action. Some appr mations have been made towards such a theory. J and Clausius have measured the velocity of gas atoms, or even determined the distance between the lision of atom and atom. Sir W. Thomson has appr mated to the number of atoms in a given bulk of stance. Rankine has formed some reasonable hypoth as to the actual constitution of atoms, but it would mistake to suppose that these ingenious results of th and experiment form any appreciable approach to a c plete solution of molecular motions. There is e reason to believe, judging from the spectra of the elem and from other reasons, that even chemical atoms are complicated structures. An atom of pure iron is prob a vastly more complicated system than that of the pla and their satellites. A compound atom may perhaps be compared with a stellar system, each star a minor system in itself. The smallest particle of solid substance will consist of a vast number of such stellar systems united in regular order, each bounded by the other, communicating with it in some manner yet wholly incomprehensible. Now what are our mathematical powers in comparison with this problem?

After two centuries of continuous labour, the most gifted men have succeeded in calculating the mutual effects of three bodies each upon the other, under the simple hypothesis of the law of gravity. Concerning these calculations we must further remember that they are purely approximate, and that the methods would not apply where four or more bodies are acting, and all produce considerable effects each upon the other. There is every reason to believe that each constituent of a chemical atom must go through an orbit in the millionth part of the twinkling of an eye, in which it successively or simultaneously is under the influence of many other constituents, or possibly comes into collision with them. It is, I apprehend, no exaggeration to say that mathematicians have scarcely a notion of the way in which they could successfully attack so difficult a problem of forces and motions. Each of these particles is for ever solving differential equations, which, if written out in full, might perhaps belt the earth, as Sir J. Herschel has beautifully remarked[m].

Some of the most extensive calculations ever made, were those required for the reduction of the measurements executed in the course of the Trigonometrical Survey of Great Britain. The calculations arising out of the principal triangulation alone occupied twenty calculators during three or four years, in the course of which the computers had to solve simultaneous equations involv seventy-seven unknown quantities. The reduction of levellings again required the solution of a system ninety-one equations. But these vast calculations pre no approach whatever to what would be requisite for complete treatment of any one physical problem. ' motion of glaciers is supposed to be moderately v understood in the present day. A glacier is a vis slowly yielding mass, neither absolutely solid nor al lutely rigid, but it is expressly remarked by Forb that not even an approximate solution of the mat matical conditions of such a moving mass can yet be sible. 'Every one knows,' he says, 'that such probl are beyond the compass of exact mathematics;' though mathematicians may know this, they do not o enough impress that knowledge on other people.

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[m] 'Familiar Lectures on Scientific Subjects,' p. 458.

The problems which are solved in our mathemat books consist of a small selection of those which hap from peculiar conditions to be practicable. But the v simplest problem in appearance will often give rise impracticable calculations. Mr. Todhunter[o] seems blame Condorcet, because in one of his memoirs he n tions a problem to solve which would require

$$n + n' + n'' + n''' - 2$$

successive integrations. Now if our mathematical scie are to pretend to cope with the problems which await s tion, we must be prepared to effect an unlimited nun of successive integrations; yet at present, and aln beyond doubt for ever, the probability that even a si integration, taken haphazard, will be found to come wi our powers is exceedingly small.

In some passages of that most remarkable work,

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[n] 'Philosophical Magazine,' 3rd Series, vol. xxvi. p. 406.

[o] 'History of the Theory of Probability,' p. 398.

'Ninth Bridgwater Treatise[p],' Mr. Babbage has pointed out that if we had power to follow and detect the minutest effects of any disturbance, each particle of existing matter must be a register of all that has happened. 'The track of every canoe—of every vessel that has yet disturbed the surface of the ocean, whether impelled by manual force or elemental power, remains for ever registered in the future movement of all succeeding particles which may occupy its place. The furrow which it left is, indeed, instantly filled up by the closing waters; but they draw after them other and larger portions of the surrounding element, and these again, once moved, communicate motion to others in endless succession.' We may even say that 'The air itself is one vast library, on whose pages are for ever written all that man has ever said or even whispered. There, in their mutable but unerring characters, mixed with the earliest, as well as the latest sighs of mortality, stand for ever recorded, vows unredeemed, promises unfulfilled, perpetuating in the united movements of each particle, the testimony of man's changeful will[q].'

When we read truthful reflections such as these, we may congratulate ourselves that we have been endowed with minds which, rightly employed, can form some estimate of their incapacity, to trace out and account for all that proceeds in the simpler actions of material nature. It ought to be added that, wonderful as is the extent of physical phenomena open to our investigation, intellectual phenomena are yet vastly more extensive. Of this I might present one satisfactory proof were space available by pointing out that the mathematical functions employed in the calculations of physical science, form an infinitely small fraction of the functions which may be invented. Common trigonometry, for instance, con of a great series of useful formulas, all of which out of the simple fundamental relation of the sine cosine expressed in the one equation

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[p] 'Ninth Bridgwater Treatise,' p. 115.

[q] Ibid. p. 113.

$$\sin^2 x + \cos^2 x = 1.$$

But this is not the only trigonometry which may e mathematicians also recognise the so-called hyper trigonometry of which the fundamental equation is

$$\cos^2 x - \sin^2 x = 1.$$

De Morgan has pointed out that the symbols of ordi algebra form but three of an interminable series of ceivable systems[a]. As the logarithmic operation is to a tion or addition to multiplication, so is the latter higher operation, and so on without limit.

We may rely upon it that indefinite, and to us in ceivable, advances will be made by the human intellec the absence of any unforeseen catastrophe to the sp or the globe. Almost within historical periods we trace the rise of mathematical science from its sim germs. We can prove our descent from ancestors counted only on their fingers, but how almost infini is a Newton or a Laplace above those simple sava Pythagoras is said to have sacrificed a hecatomb w he discovered the Forty-seventh Proposition of Euclid, the occasion was worthy of the sacrifice. Archimedes beside himself when he first perceived his beautiful n of determining specific gravities. Yet these great coveries are the simplest elements of our schoolboy-k ledge. Step by step we can trace upwards the acquiren of new mental powers. What could be more wond and unexpected than Napier's discovery of logarithn wholly new mode of calculation which has multi perhaps a hundred-fold the working powers of e computer, and indeed has rendered easy calculations w were before almost impracticable. Since the time of Newton and Leibnitz whole worlds of problems have been solved which before were hardly conceived as matters of inquiry. In our own day extended methods of mathematical reasoning, such as the system of quaternions, have been brought into existence. What intelligent man will doubt that the recondite speculations of a Cayley or a Sylvester may possibly lead to some new methods, at the simplicity and power of which a future age will wonder, and yet wonder more that to us they were so dark and difficult. May we not repeat the words of Seneca: 'Veniet tempus, quo ista quæ nunc latent, in lucem dies extrahat, et longioris ævi diligentia: ad inquisitionem tantorum ætas una non sufficit. Veniet tempus, quo posteri nostri tam aperta nos nescisse mirentur.'

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[a] 'Trigonometry and Double Algebra,' chap. IX.

The Reign of Law in Mental and Social Phenomena.

After we pass from the so-called physical sciences to those which attempt to investigate mental and social phenomena, the same general conclusions will hold true. No one will be found to deny that there are certain uniformities of thinking and acting which can be detected in reasoning beings, and so far as we detect such laws we successfully apply scientific method. But those who attempt thus to establish social or moral sciences, soon become aware that they are dealing with subjects of enormous perplexity. Take, for instance, the science of Political Economy. If a science at all, it must be a mathematical science, because it deals with quantities of commodities. But so soon as we attempt to draw out the equations expressing the laws of variation of demand and supply, we discover that they must have a complexity entirely surpassing our powers of mathematical treatment.

We may lay down the general form of the equations, pressing the demand and supply for two or three com dities among two or three trading bodies, but all functions involved are of so complicated a character there is not much fear of scientific method makin rapid progress in this direction. If such be the prosp of a comparatively formal science, like Political Econo what shall we say of Moral Science? Any comp theory of morals must deal with quantities of pleas and pain, as Bentham pointed out, and must sum up general tendency of each kind of action upon the g of the community. If we are to apply scientific met to morals, we must have a calculus of moral effect kind of physical astronomy investigating the mutual turbations of individuals. But as astronomers have yet fully solved the problem of three gravitating bod when shall we have a solution of the problem of th moral bodies?

Now the sciences of political economy and morality comparatively, abstract and general, treating mank from simple points of view, and attempting to de general grounds of action. They are to social phenom what the general sciences of chemistry, heat, and ele city, are to the concrete science of meteorology. Be we can investigate the actions of any aggregate of n we must have fairly mastered all the more abst sciences applying to them, somewhat in the way t we have acquired a fair comprehension of the sim truths of chemistry and physics. But all our phys sciences do not enable us to predict the weather two d hence with any great probability, and the general prob of meteorology is almost unattempted as yet. What s we say then of the general problem of social science, wl shall enable us to predict the course of events in a nati

There have indeed been several writers who posed to lay the foundations of the science of history. The late Mr. Buckle undertook to write the 'History of Civilisation in England,' and showed how the character of a nation could be explained by the nature of the climate and the fertility of the soil. He omitted to explain the contrast between the ancient Greek nation and the present one; either there must have been an extraordinary revolution in the climate and the soil, or some more complex causes must be imagined to have come into operation. Auguste Comte detected some very fundamental and simple laws of development through which nations pass. There are always three phases of intellectual condition,—the theological, the metaphysical, and the positive; and applying this general law of progress to concrete cases, Comte was enabled to predict that in the hierarchy of European nations, Spain would necessarily hold the highest place. Such are the parodies of science offered to us by the so-called positive philosophers.

A science of history in the true sense of the term is an absurd notion. A nation is not a mere sum of individuals whom we can treat by the method of averages; it is an organic whole, held together by ties of infinite complexity. Each individual acts and re-acts upon his own smaller or greater circle of friends, and those who acquire a public position, exert an influence on much larger sections of the nation. There will always be a few great leaders of exceptional genius or opportunities, the unaccountable phases of whose opinions and inclinations sway the whole body, even when they are least aware of it. From time to time arise critical positions, battles, delicate negotiations, internal disturbances, in which the slightest incidents may profoundly change the course of history. A rainy day may hinder a forced march, and change the course of a campaign; a few in-effects of a constantly acting tendency. Even man, according to these theories, is no distinct creation, but rather an extreme specimen of brain development. His nearest cousins are the apes, and his pedigree extends backwards until it joins that of the lowliest zoophytes.

The theories of Darwin and Spencer are doubtless not demonstrated; they are to some extent hypothetical, just as all the theories of physical science are to some extent hypothetical, and open to doubt. But I venture to look upon the theories of evolution and natural selection in their main features as two of the most probable hypotheses ever proposed, harmonizing and explaining as they do immense numbers of diverse facts. I question whether any scientific works which have appeared since the 'Principia' of Newton, are comparable in importance with those of Darwin and Spencer, revolutionizing as they do all our views of the origin of bodily, mental, moral, and social phenomena.

Granting all this, I cannot for a moment admit that the theory of evolution will alter our theological views. That theory embraces several laws or uniformities which are observed to be true in the production of living forms; but these laws do not determine the size and figure of living creatures, any more than the law of gravitation determines the magnitudes and distances of the planets. Suppose that Darwin is correct in saying that man is descended from the Ascidians: yet the precise form of the human body must have been influenced by an infinite train of circumstances affecting the reproduction, growth, and health of the whole chain of intermediate beings. No doubt, the circumstances being what they were, man could not be otherwise than he is, and if in any other part of the universe an exactly similar earth, furnished, with exactly similar germs of life, existed, a race must have grown up there exactly similar to the human race.

By a different distribution of atoms in the prim
world a different series of living forms on this earth
have been produced. From the same causes acting ac
ing to the same laws, the same results will follow ;
from different causes acting according to the same l
different results will follow. So far as we can see, t
infinitely diverse living creatures might have been
ated consistently with the theory of evolution, and
precise reason why we have a back-bone, two hands
opposable thumbs, an erect stature, a complex brain, a
223 bones, and many other peculiarities, is only t
found in the original act of creation. I do not, any
than Paley, believe that the eye of man manifests de
I believe that the eye was gradually developed, and
can in fact trace its gradual development from the
germ of a nerve affected by light rays in some si
zoophyte. In proportion as the eye became a
delicate and accurate instrument of vision, it enable
possessor to escape destruction, but the ultimate re
must have been contained in the aggregate of the ca
and these causes, so far as we can see, were subje
the arbitrary choice of the Creator.

Although Professor Agassiz is clearly wrong in hol
that every species of animals or plants has appeare
earth by the immediate intervention of the Creator, w
would amount to saying that no laws of connexion
tween forms are discoverable, yet he seems to be righ
asserting that living forms are entirely distinct from t
produced from purely physical causes. 'The produc
what are commonly called physical agents,' he says[u],
everywhere the same (*i. e.* upon the whole surface of
earth) and have always been the same (*i. e.* during all
logical periods); while organized beings are everywhere
ferent and have differed in all ages. Between two such s

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[u] Agassiz, 'Essay on Classification,' p. 75.

of phenomena there can be no causal or genetic connexion.' Living forms as we now regard them are essentially variable. Now from constant mechanical causes constant effects would ensue. If vegetable cells are formed on geometrical principles, being first spherical, and then by mutual compression dodecahedral, then all cells should have similar forms. In the Foraminifera and some other of the more lowly organisms, we do seem to observe the production of complex forms on pure geometrical principles. But from similar causes acting according to similar laws and principles, only similar results could be produced. If the original life-germ of each creature is a simple particle of protoplasm, unendowed with any distinctive forces, then the whole of the complex phenomena of animal and vegetable life are effects without causes. Protoplasm may be chemically the same substance, and the germ-cell of a man and of a fish may be apparently the same, so far as the microscope can decide; but if certain cells produce men and others as uniformly produce a given species of fish, there must be a hidden constitution determining the extremely different results. If this were not so, the generation of every living creature from the uniform germ would have to be regarded as a distinct act of arbitrary creation.

Theologians have dreaded the establishment of the theories of Darwin and Spencer, as if they thought that those theories could explain everything upon the purest mechanical and material principles, and exclude all notions of design. They do not see that those theories have opened up more questions than they have closed. The doctrine of evolution gives a complete explanation of no single living form. While showing the general principles which prevail in the variation of living creatures, it only points out the infinite complexity of the causes and circumstances which have led to the present state of things.

Any one of Mr. Darwin's books, admirable though the are, consists but in the setting forth of a multitud indeterminate problems. He proves in the most beau manner that each flower of an orchid is adapted to s insect which frequents and fertilizes it, and these ada tions are but a few cases of those immensely nume ones which have occurred throughout the life of pl and animals. But why orchids should have been for so differently from other plants, why anything, ind should be as it is, rather than in some of the other finitely numerous possible modes of existence, he never show. The origin of everything that exist wrapped up in the past history of the universe. some one or more points in past time there must been arbitrary determinations which led to the pro tion of things as they are.

Possibility of Divine Interference.

I will now draw the reader's attention to pages 168- of the first volume. I there pointed out that all induc inference involves the assumption that our knowledg what exists is complete, and that the conditions of th remain unaltered between the time of our experience the time to which our inferences refer. Recurring to illustration of a ballot-box, employed in the Chapte the Inverse Method of Probabilities, we assume w predicting the probable nature of the next drawing, our previous drawings have been sufficiently numerou give us nearly complete knowledge of the contents of box; and, secondly, that no interference with the ba box takes place between the previous and the next d ings. The results yielded by the theory of probabil are quite plain. No finite number of casual drawings give us sure knowledge of the contents of the box, so t even in the absence of all disturbance, our inferences are merely the best which can be made, and do not approach to infallibility. If, however, interference be possible, even the theory of probability ceases to be applicable, for, the amount and nature of that interference being arbitrary and unknown, there ceases to be any connexion between premises and conclusion. Many years of reflection have not enabled me to see any way of avoiding this hiatus of scientific certainty. The conclusions of scientific inference appear to be always of an hypothetical and purely provisional nature. Given certain experience the theory of probability yields us the true interpretation of that experience and is the surest guide open to us. But the best calculated results which it can give are never absolute probabilities; they are purely relative to the extent of our information. It seems to be impossible for us to judge how far our experience gives us adequate information of the universe as a whole, and of all the forces and phenomena which can have place therein.

I feel that I cannot in the space remaining at my command in the present volume, sufficiently follow out the lines of thought suggested, or define with precision my own conclusions. This chapter contains merely *Reflections* upon subjects of so weighty a character that I should myself wish for many years—nay for more than a lifetime of further reflection. My purpose, as I have repeatedly said, is the purely negative one of showing that atheism and materialism are no necessary results of Scientific Method. From the preceding reviews of the value of our scientific knowledge, I draw one distinct conclusion, that we cannot disprove the possibility of Divine interference in the course of nature. Such interference might arise, so far as our knowledge extends, in two ways. It might consist in the disclosure of the existence of some agent or spring of energy previously unknown, but which effects a dictory results. The very nature of continuous quantity leads us into extreme difficulties. Any finite length is composed of an infinite number of infinitely small spaces, each of which, again, is composed of an infinite number of spaces of a second order of infinite smallness; these spaces of the second order are composed, again, of infinitely small spaces of the third order. Even these spaces of the third order are not absolute geometrical points answering to Euclid's definition of a point, as position without magnitude. Go on as far as we will, in the subdivision of continuous quantity, yet we never get down to the absolute point. Thus Scientific Method leads us to the inevitable conception of an infinite series of successive orders of infinitely small quantities. If so, there is nothing impossible in the existence of a myriad universes within the compass of a needle's point, each with its stellar systems, and its suns and planets, in number and variety unlimited. Science does nothing to reduce the number of strange things that we may believe. When fairly pursued it makes large drafts upon our powers of comprehension and belief.

Some of the most precise and beautiful theorems in mathematical science seem to me to involve apparent contradiction. Can we imagine that a point moving along a perfectly straight line towards the west, would ever get round to the east and come back again, having performed a circuit through infinite space, as it were, yet without ever diverging from a perfectly straight direction? Yet this is what happens to the intersecting point of two straight lines, when, being in the same plane, one line revolves about a fixed point. The same principle is exhibited in the hyperbola, which may be regarded as an infinite ellipse, one extremity of which has passed to an infinite distance and come back in the opposite direction. A varying quantity may change that the wonders and subtleties of possible existence surpass all that our mental powers allow us clearly to perceive. The study of abstract logical and mathematical forms has seemed to convince me that even space itself is no requisite condition of conceivable existence. Everything, we are told by materialists, must be here or there, nearer or further, before or after. I deny this—and point to logical relations as my proof.

There formerly seemed to me to be something highly mysterious in the denominators of the binomial expansion (vol. i. p. 216) which are reproduced in that strange natural constant *e*, or

$$1+\frac{1}{1}+\frac{1}{1\cdot2}+\frac{1}{1\cdot2\cdot3}+\dots\dots$$

and in many results of mathematical analysis. I now perceive, as already partially explained (vol. i. pp. 40–42, 180, 181, 443, 444), that they arise out of the fact that the relations of space do not apply to the logical conditions which govern the numbers of combinations as contrasted to those of permutations. So far am I from accepting Kant's doctrine that space is a necessary form of thought, that I regard it as an accident, and an impediment to pure logical reasoning. Material existences must exist in space no doubt, but intellectual existences may be neither in space nor out of space; they may have no relation to space at all, just as space itself has no relation to time. For all that I can see, then, there may be intellectual existences to which both time and space are nullities.

Now among the most unquestionable rules of Scientific Method is that first law that whatever phenomenon is, is. We must ignore no existence whatever; we may variously interpret or explain its meaning and origin, but if a phenomenon does exist it demands some kind of explanation. If then there is to be [illegible] is recog-

nition, the world without us must y
existence of the spirit within. Our
and determinations are the most u
within the sphere of consciousness.
and live as if they were not merely
casual conjunction of atoms, but the
reaching purpose, are we to record al
pass over these? We investigate t
and the bee and the beaver, and disc
by an inscrutable agency to work t
pose. Let us be faithful to our
investigate also those instincts of
which man is led to work as if the
Being were the aim of life.

THE END.

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